7

THERMODYNAMIC ANALYSIS OF Pd-D ELECTROCHEMICAL CELLS

- Heat Energy From Calorimeter Is Not Equal To Electrical Energy Into The Electrochemical Cell.
- Voltage Of Cell Is Equivalent To The Free Energy (ΔG_r), But Calorimeter Measures The Enthalpy (ΔH_r),
  - (at small currents and work dissipated inside).
- For Electrical Work Dissipated Inside The Calorimeter:
  - Electrochemical Energy = $W_e = -zFE^o = \Delta G_r$
  - Calorimeter Energy = $q_T = -zFE_H = \Delta H_r$
- Difference Equals The Entropy Term,
  - $-T\Delta S_r = -zFT\partial E^o/\partial T$.

ELECTROCHEMICAL/ THERMODYNAMIC CONVERSIONS

1). $\Delta G_r = \Delta H_r - T\Delta S_r$

2). $-\Delta G_r/zF = -\Delta H_r/zF + T\Delta S_r/zF$

3). $E^o = E_H + E_S$

4). $E^o = E_H + T\partial E^o/\partial T$

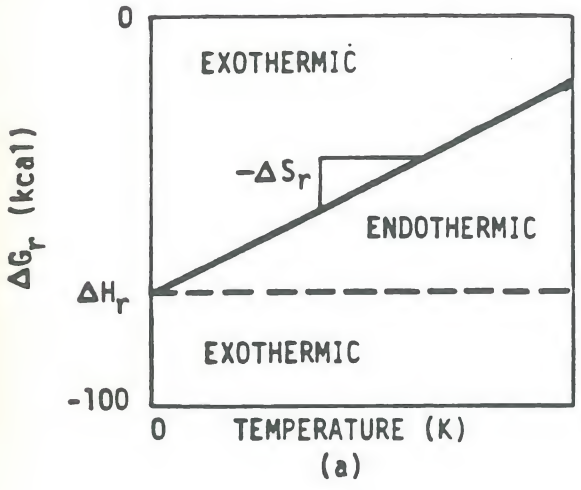
- For Electrical Work Dissipated Outside The Calorimeter, And Including a Term for Irreversible Overpotentials:
  - Calorimeter Energy = $q_T = q_p + q_s = -zF(E_H - E_L)$
- Calorimetric Heat Depends on E_H, the 'Thermoneutral Potential' -- not on E^o, the ΔG_r Term.
  - Vaporization Is Included In $E_H = 1.53$ V Term.

GALVANIC OPERATION OF AN ELECTROCHEMICAL CELL

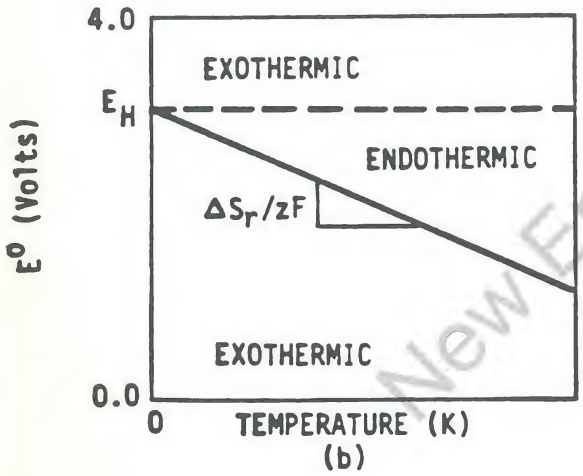
CASE I

SPONTANEOUS REACTION (DISCHARGE) HAS A NEGATIVE ENTROPY CHANGE

I. TOTAL : $A + B \rightleftharpoons C + D$



(a)



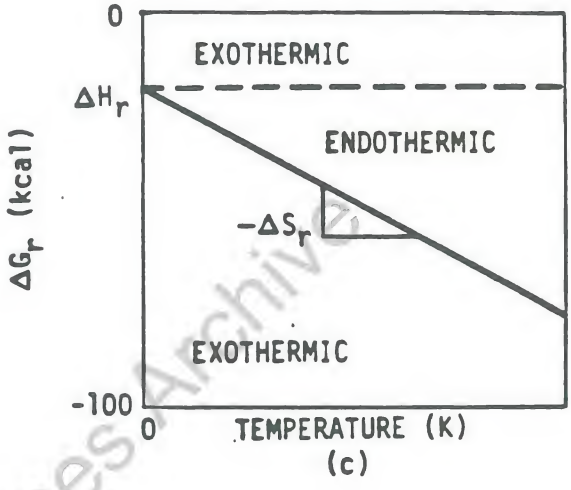
(b)

PRODUCTS OF BATTERY REACTION (DISCHARGE) ARE MORE ORDERED THAN REACTANTS.
FOR THE REACTION IN THE DISCHARGE DIRECTION, ΔS_r IS NEGATIVE, THE SLOPE OF ΔG_r vs T IS POSITIVE, AND THE SLOPE OF E° vs T IS NEGATIVE.
OPEN-CIRCUIT VOLTAGE (E°) IS LESS THAN THE THERMONEUTRAL POTENTIAL (E_H) AT ALL TEMPERATURES.
BATTERY DISCHARGE GENERATES HEAT AT ALL CURRENTS.
ZERO HEAT FLOW WOULD OCCUR AT SOME POINT DURING BATTERY CHARGE.

CASE II

SPONTANEOUS REACTION (DISCHARGE) HAS A POSITIVE ENTROPY CHANGE

II. TOTAL : $A + B \rightleftharpoons C + D$



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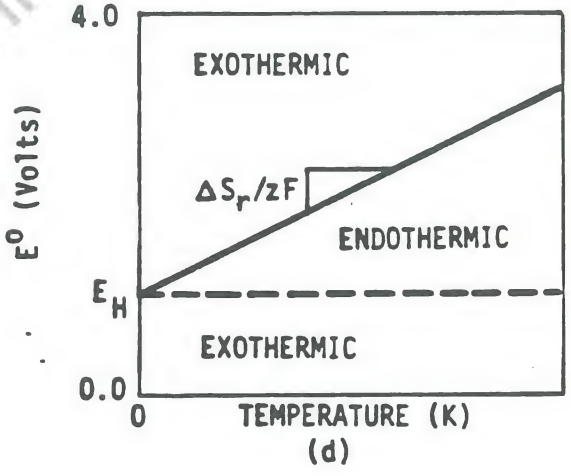


(d)

1. REACTANTS OF BATTERY REACTION (DISCHARGE) ARE MORE ORDERED THAN PRODUCTS.
2. FOR THE REACTION IN THE DISCHARGE DIRECTION, ΔS_r IS POSITIVE, THE SLOPE OF ΔG_r vs T IS NEGATIVE, AND THE SLOPE OF E° vs T IS POSITIVE.
3. OPEN-CIRCUIT VOLTAGE (E°) IS GREATER THAN THE THERMONEUTRAL POTENTIAL (E_H) AT ALL TEMPERATURES.
4. BATTERY CHARGE GENERATES HEAT AT ALL CURRENTS.
5. ZERO HEAT FLOW WOULD OCCUR AT SOME POINT DURING BATTERY DISCHARGE .

Figure 3

TWO CALORIMETRIC ANALYSIS METHODS

- **Thermoneutral Potential Method:** $\dot{q}_T = -I(E_H - E_L)$
  - Expected Calorimetric Heat Rate from Power Input To the Cell Is Given By $\dot{q}_T = -I(E_H - E_L)$, If No Gases Recombine in the Calorimeter.
  - Most Correct Method To Measure Electrochemical Reactions Calorimetrically.

- **Electrical Work Method:** $\dot{q}_T = -I(-E_L) = I \cdot E_L$
  - Expected Calorimetric Heat Rate from Power Input To the Cell Is Given By $\dot{q}_T = I \cdot E_L$, If All Gases Recombine in the Calorimeter.
  - Most Conservative Method, If Some Gases Recombine.
  - Ideally, Would Empirically Determine Fraction.

- **Heat From PdD_x Formation Must Also Be Included:**
  - $\Delta H_r = -36.8$ kJ/mole (+0.191 V); $\Delta G_r = -6.0$ (+0.031 V)
  - This Represents Only 0.02% of the 20 W/cm^3 Reported.

Example: Calorimetrically Expected Energies

Cell Voltage = −5 Volts
Cell Current = 1.0 Ampere
Time = 1 week
E_H = 1.527 Volts

	Method	
	Thermoneutral Potential	Electrical Work
Analytical Expression Pons & Fleischmann Terms	$(E_H - E_L)$ Joule Heating (a)	(E_L) Total Energy (b)
Power Measured (W)	−3.48	−5.00
(Plus PdD_x Formation)	(−3.66)	
Energy Measured (MJ)	−2.10	−3.02
Difference Between Methods	~30%	--

Energy Scales

Voltage X zF (Volts)		Thermochemical (kJ/mole)
+0.191	E_H = Enthalpy of Formation of PdD_x	−36.80
+0.031	E^o = Free Energy of Formation of PdD_x	−5.98
	E^o = Electrochemical Fraction of Water Electrolysis	
−1.262	E^o = Free Energy of Formation of D_2 & O_2 Gases	+243.44
	E_S = Endothermic Fraction of Water Electrolysis	
−1.527	E_H = Thermoneutral Potential of Water Electrolysis	+294.60
	Cell Polarization Heating	
−5.00	E_L = Cell Voltage	+964.87

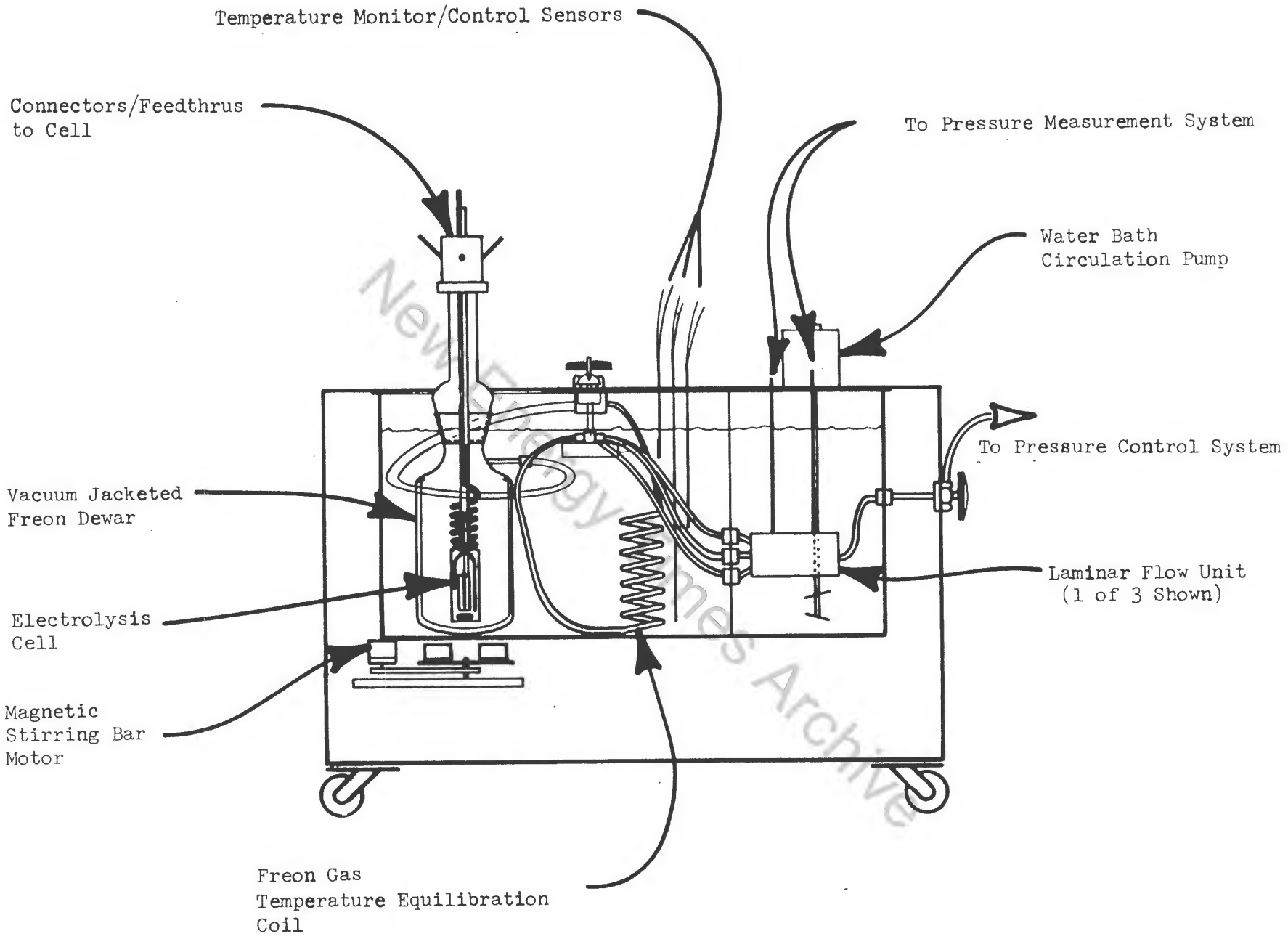
SANDIA FREON-VAPORIZATION CALORIMETER

- **Freon-Vaporization Calorimeter:**
  - **Cell Heat Given By Flow of Boiled Freon (19°C).**
  - **Does Not Depend on Temperature Measurement in Cell.**
  - **Cell Temperature Provides Independent Check.**
  - **Both D_2O and Freon Are Stirred to Minimize $\partial T/\partial x$.**
  - **Software Developed for Continuous Collection of Data.**

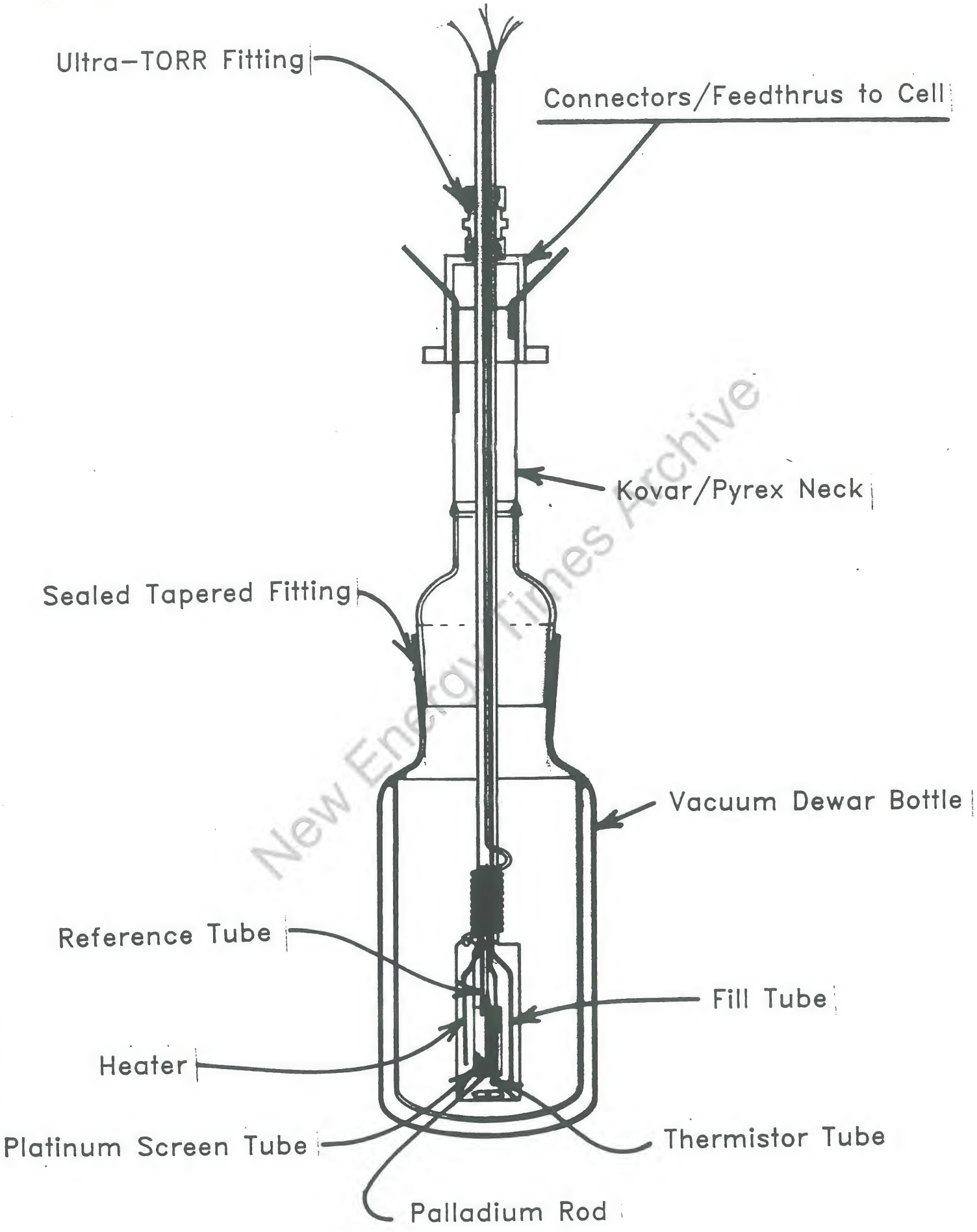
- **Simultaneous Measurement With Four High-Sensitivity Neutron Detectors (see also Poster Presentation).**

- **Precision of 0.1 Watts:**
  - **~2% of Electrical Input Power; ~0.4% of 20 W/cm^3 Pd.**
  - **Large Calorimeter: Can Measure 40 Watts of Power.**

- **Electrochemical Cell: Cast Pd Rod/0.1 M LiOD, D_2O/Pt**
  - **Incorporates Pt Reference Electrode.**



FREON VAPORIZATION CALORIMETER



CONCLUSIONS

- Energy Measured From Calorimeter Is Not Equal To The Electrical Energy Supplied To It.
- The Correct Calorimetrically-Measured Power Expected From an Electrochemical Reaction Is Given By $\dot{q}_T = -I\,(E_H - E_L)$.
- However, If D_2 & O_2 Gases Recombine To Any Appreciable Extent, Then the Most Conservative Heating Power Expected Is Given By $\dot{q}_T = I \cdot E_L$.
- The Electrolysis Of D_2O Is Endothermic, and Formation Of D_2 & O_2 Gases Requires ~30% of the Input Energy.
- The $P\Delta V$ Vaporization of the D_2 and O_2 Gases Is Included in the 1.53 V Thermoneutral Potential Term.

CONCLUSIONS (cont'd)

- The Enthalpy of PdD_x Formation, Although Small, Also Generates Some Chemical Heat, And Reduces Any Observed 'Excess Heats' in a Conservative Analysis.
  - 0.02% of the 20 W/cm^3 Reported by Utah.
  - Similar Value for the Insertion of Li into Pd.

- Sandia Freon-Vaporization Calorimeter:
  - Large Calorimeter: Can Measure Large Pd Electrodes At Large Current Densities, as suggested by Utah.
  - Can Measure 40 Watts with Precision of 0.1 W (<0.5%).
  - Simultaneous Measurement of Neutrons.
  - All Heats Integrated Over Time; Not Based on Temp.

CALORIMETRIC AND THERMODYNAMIC ANALYSIS OF PALLADIUM-DEUTERIUM ELECTROCHEMICAL CELLS

N. A. Godshall, E. P. Roth, M. J. Kelly,
T. R. Guilinger, and R. I. Ewing
Sandia National Laboratories

- Thermodynamic Analysis of Input Energies
- Expected Heats from Calorimetric Measurements
- Electrochemical Considerations

"He told me that Dr. Pons would try to come," Dr. Redish said. "But just

Tens of millions of dollars at stake, Dear Brother.

Physicists Debunk Claim Of a New Kind of Fusion

By MALCOLM W. BROWNE
Special to The New York Times

BALTIMORE, May 2 — Hopes that a new kind of nuclear fusion might give the world an unlimited source of cheap energy appear to have been dealt a devastating blow by scientific evidence presented here.

In two days of meetings lasting until midnight, members of the American Physical Society heard fresh experimental evidence from many researchers that nuclear fusion in a jar of water does not exist.

Physicists seemed generally persuaded as the sessions ended that assertions of "cold fusion" were based on nothing more than experimental errors by Utah scientists.

Furor on Initial Claim

Dr. B. Stanley Pons, professor of chemistry at the University of Utah, and his colleague, Dr. Martin Fleischmann of the University of Southampton in England, touched off a furor by asserting on March 23 in Salt Lake City that they had achieved nuclear fusion in a jar of water at room temperature.

At a news conference today, nine of the leading speakers were asked how many would now rule the Utah claim as dead. Eight said yes, and only one, Dr. Johann Rafelski of the University of Arizona, withheld judgment.

Top physicists directed angry attacks at Dr. Pons and Dr. Fleischmann, calling them incompetent, reciting sarcastic verses about their claims and complaining that they had refused to provide details needed for follow-up experiments. A West European expert said that "essentially all" West European attempts to duplicate cold fusion had failed.

Response at Utah University

In a telephone interview, Dr. James Brophy, director of research at the University of Utah, responded, "It is difficult to believe that after five years of experiments Dr. Pons and Dr. Fleischmann could have made some of the errors I've heard have been alleged at the American Physical Society meeting."

The criticism at the regular spring meeting of the society came just before Dr. Pons was scheduled to meet with representatives of President Bush and just after the University of Utah asked Congress to provide $25 million to pursue Dr. Pons's research. A university spokesman said Dr. Pons was in Washington and could not be reached to answer questions.

Cold fusion, Dr. Pons and Dr. Fleischmann said, can be initiated in a cell containing heavy water, in whose molecules the heavy form of hydrogen called deuterium is substituted for ordinary hydrogen. When current is

Continued on Page A14, Column 1

sought to repeat the cold fusion experiments, and some completed their investigations just hours before the

that someone turned the cur... off for a while. When that happens hydrogen naturally bubbles out of the palladium

'HE NEW YORK TIMES **NATIO**

Associated Press

...l Physical Society, Walter Meyerhof of ...h of an experiment in which University ... had created nuclear fusion in a jar of ...ers heard fresh experimental evidence ...h nuclear fusion does not exist.

...that ...oyed ...said, ...t the ...stand

ment, questioning was generally friendly.

He drew cheers and laughter when he concluded his talk by saying, "Is this a short cut to fusion energy? Read my lips: No!" He defended his own experiment, describing his results as a "fragile flower" that would never grow

Dr. Pons would try
ish said. "But just

Tens of millions of dollars at stake, Dear Brother,

Fusion Claim Meets Scorn Of Physicists

Continued From Page 1

passed through the heavy water from a palladium cathode, the Utah team said, the palladium absorbs deuterium atoms, which are forced to fuse together, generating heat and neutrons.

Fusion, which powers the sun and hydrogen bombs, normally occurs only at extremely high temperatures. If a means could be found to harness a form of hydrogen fusion as a commercial source of power, some scientists have said, energy shortages could be forestalled.

Some of the new experiments also sought to reproduce the less contentious findings on cold fusion reported independently by Dr. Steven E. Jones and his colleagues at Brigham Young University in Utah. Dr. Jones, who used a device similar to the one in the Pons-Fleischmann experiment, did not claim that any useful energy was produced. But he did report that slightly more neutrons were detected while the cell was operating than could be expected from normal sources. The result suggests at least the possibility of fusion, he said, although it is not likely to be useful as an energy source.

Physicists who have investigated Dr. Jones's report have been fairly restrained in their criticism, acknowledging that Dr. Jones is a careful scientist. But from the outset they have expressed profound skepticism of claims by Dr. Fleischmann and Dr. Pons.

Attempts to Repeat Experiments

Since March, scores of laboratories in the United States and abroad have sought to repeat the cold fusion experiments, and some completed their investigations just hours before the

Associated Press

At the meeting of the American Physical Society, Walter Meyerhof of Stanford University held a sketch of an experiment in which University of Utah researchers said they had created nuclear fusion in a jar of water. At the meeting, members heard fresh experimental evidence from many researchers that such nuclear fusion does not exist.

could account for the burst of heat that Dr. Pons reported as having destroyed one of the Utah cells.

"My understanding," Dr. Lewis said, "is that Pons's son was there at the time, not Pons himself. I understand that someone turned the current off for a while. When that happens hydrogen naturally bubbles out of the palladium

ment, questioning was generally friendly.

He drew cheers and laughter when he concluded his talk by saying, "Is this a short cut to fusion energy? Read my lips: No!" He defended his own experiment, describing his results as a "fragile flower" that would never grow

Ottenuta in una università la stessa energia del primo test di Pons e Fleishmann

Il Sole – 24 Ore

Anche in Texas riesce la fusione fredda

Una prima parziale verifica dell'esperimento di «fusione fredda» di Pons e Fleishmann è venuta ieri dagli scienziati del Texas Agricoltural And Mechanical University di College Station. Il nuovo esperimento, ha spiegato ieri nel corso di una conferenza stampa il professor Charles Martin, ha prodotto la medesima quantità (piuttosto sensibile) di energia rispetto alle prove di Pons e Fleishmann ma non ha affatto confermato che l'origine di tale energia sia proprio una fusione nucleare.

«È stato confermato solo un aspetto, la produzione di energia», ha detto Martin, professore associato di chimica che ha svolto l'esperimento di verifica insieme a un gruppo di ricercatori di elettrodinamica. Ma prima di attribuire questa produzione energetica alla fusione di nuclei di deuterio, ammoniscono i ricercatori, saranno necessarie altre verifiche.

«Mi sentirei molto più a mio agio se avessimo verificato la fusione nucleare — ha rilevato Martin — ma ancora non ci siamo». L'università texana ha comunque avviato diversi esperimenti per verificare la «fusione fredda». La prova riuscita era stata avviata venerdì scorso ed è andata avanti per 40 ore.

Ieri lo stesso Martin ha annunciato al telefono a Pons e Fleishmann i risultati del suo esperimento. I due chimici avrebbero espresso il loro «grande sollievo» per la ripetizione, almeno parziale, dei loro stessi risultati.

Uno degli scienziati che ha realizzato la verifica nel Texas, Kenneth Marsh, ha spiegato che il metodo utilizzato per misurare l'energia prodotta è diverso da quello utilizzato da Pons e Fleishmann. L'energia è stata ottenuta sotto forma di calore e successivamente è stata trasformata in energia elettrica. Ma è stata prodotta in quantità notevole, assicurano gli scienziati texani.

«I risultati annunciati dagli scienziati della Texas Agricultural and Mechanical University sono molto incoraggianti — ha dichiarato poi lo stesso Stanley Pons in un comunicato — e sono quasi esattamente gli stessi che abbiamo annunciato»

Secondo Pons l'esempio dell'università texana spingerà altri scienziati a riprodurre il fenomeno in tutto il mondo.

Sia Pons e Fleishmann che i ricercatori texani hanno utilizzato catodi in palladio immersi in contenitori di acqua pesante, ricca di deuterio e trizio. L'elettrolisi in entrambi i casi ha innescato la produzione di energia al ritmo, secondo Pons e Fleishmann, di almeno quattro watt per ogni watt immesso nella cella.

Nel caso dell'esperimento texano però almeno finora non sono state registrate prove, inequivocabili di un'avvenuta fusione nucleare, come la produzione di raggi gamma, emissione di neutroni e produzione di trizio (rilevati invece da Pons e Fleishmann). Esiste quindi ancora il possibile sospetto che si potrebbe essersi trattato di una anomala forma di reazione chimica — hanno rilevato i ricercatori dell'università texana — anche se a questo punto l'ipotesi è piuttosto remota, «ma che comunque andrà verificata da ulteriori indagini e verifiche» ha aggiunto Marsh.

L'equipe texana conta comunque di incontrarsi con Pons e Fleishmann tra qualche giorno a Dallas nell'ambito di un convegno sulla chimica. I due scienziati dell'università dello Utah hanno già lasciato intendere che esamineranno approfonditamente i risultati texani.

tesa ... chiave giusta per ...

Received from S.E. Kovacs 04/29/89

Al recente convegno di Erice, il 12.4, è stato fatto il punto della situazione. In particolare i responsabili di quattro importanti laboratori americani: IBM, Bell Telephone, AT & T e Brookhaven hanno riportato di non avere ancora ottenuto alcun risultato dopo tempi dell'ordine di due, tre settimane. Harwell, che ha fatto un grossissimo sforzo, avvalendosi anche della consulenza di Fleishman, non ha dichiaratamente ancora ottenuto alcun successo, come pure gli altri laboratori europei.

Il 10.4 è comunque apparso il press release del Georgia Institute of Technology di Atlanta nel quale si riporta osservazione di neutroni (600 conteggi per ora contro un fondo di 40) in un esperimento tipo Fleishman e Pons. Viene anche accennato un metodo di trattamento del palladio che dovrebbe ridurre i tempi di caricamento del deuterio.

4. ESPERIMENTO DI FRASCATI (CON TITANIO E DEUTERIO) IN FASE GASSOSA.

Si è partiti dall'idea di verificare la possibilità di ottenere reazioni deuterio deuterio nel reticolo di un metallo (titanio, perchè disponibile) a forte affinità per l'idrogeno ed i suoi isotopi, esponendo della spugna di titanio ad atmosfere di deuterio a pressione e temperature variabili.
E' stata misurata l'emissione di neutroni, a livelli decisamente superiori al fondo (da 1 a 3 ordini di grandezza) in un esperimento di interazione tra gas deuterio e titanio, con emissioni dell'ordine di 10^{3} n/s.
Le condizioni in cui è stato possibile realizzare la misura si riferiscono a due insiemi di parametri termodinamici alquanto diversi:

1. deuterio a $\sim$40 bar a contatto col titanio a temperature variabili tra $\sim$ 77 k e $\sim$150 k, in presenza di un reticolo presumibilmente non occupato da deuterio (quasi assenza di idruro).

2. Nella condizione opposta, cioè presumibilmente in presenza di idruro, con pressione molto bassa ($\sim 10^{-2}$ torr) e temperatura che risale verso la temperatura ambiente.

E' ragionevole ritenere che i fenomeni osservati siano correlati a una transizione di fase, nei due versi, del sistema titanio deuterio. Non è da escludere che possano verificarsi situazioni del tipo "sovrasaturazione", che potrebbero giustificare una transizione improvvisa in un certo intorno.

Al momento attuale non si sono ancora individuati i fenomeni microscopici che possono spiegare un'eventuale reazione con emissione di neutroni.

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Floyd R. Newman Laboratory of Nuclear Studies

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607- 255-4397

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Telefax: 607-254-4552

April 27, 1989

Congressman Robert A. Rowe
Chairman, House Committee on Science, Space and Technology
U.S. House of Representatives
2321 Rayburn Building
Washington, DC 20515

Dear Congressman Rowe:

I read in the **New York Times** of April 27 that there has been a major debate on cold fusion before your committee. I am alarmed by the idea that your committee may authorize large funds for a commercialization of "room temperature fusion", on the basis of experiments which are highly controversial.

As you presumably have heard, many reputable laboratories have tried to duplicate the results of the experimenters at the University of Utah, some with slight success, and others without any success. The process is, to say the least, very unclear. It needs a great deal of research before any steps toward commercialization would be indicated.

The fusion of two deuterons (nuclei of heavy hydrogen) has been investigated very thoroughly since 1933, and the theory is well understood. You first have to bring two deuterons into close contact, and then reactions will take place. Half of these reactions lead to the emission of a neutron, the other half to that of a proton (nucleus of light hydrogen). Any other nuclear reactions have negligible probability by comparison.

Once the deuterons are brought together, the nuclear reactions are independent of the method by which they were brought in contact. The crystal lattice in which the deuterons are confined may have some influence in bringing them together, as the Utah scientists appear to have shown, but once they are together, it cannot influence the nuclear reaction.

Therefore, we must consider the emission of neutrons as the indispensible sign that the deuterons have fused. Neutrons were observed by the chemists of the University of Utah, and also by the physicists of Brigham Young University. But their numbers are very, very small in both cases, and at present totally insufficient to support a commercial process. Drs. Pons and Fleischmann of the University of Utah claim that they observe energy evolution about a billion times greater than would be produced by the nuclear reactions producing neutrons. Where this energy comes from is at present totally unknown, but it cannot come from fusion. The fusion reaction is far too well studied and understood for this to be the case.

That neutrons are produced at room temperature is a surprising result, and deserves to be further investigated. Also, the production of large amounts of heat should be subject to intensive research. Therefore, it may be indicated that the two research teams in Utah be supported by a research grant of the order of $1,000,000 for one year to further their fundamental research and, as far as possible, to clarify the many remaining questions. If after this research the process looks promising, then the time may come to give them support for commercialization, but not now. I am happy to read in the **New York Times** report that the scientists of the University of Utah are collaborating with the Los Alamos National Laboratory, one of the most expert laboratories in nuclear physics in the world.

Concerning my own qualifications, I received the Nobel Prize in Physics in 1967 for my work on the theory of nuclear reactions. In 1936-37 I wrote a series of three articles in the **Reviews of Modern Physics** on nuclear theory and nuclear reactions, which remained the standard text for about twenty years, and is still consulted. I am still actively working in nuclear science, and I am a consultant to the Los Alamos National Laboratory on both classified and unclassified problems.

It would be a black mark against nuclear science, and against your committee, if you rushed into this unproven technology prematurely. Nuclear fission was discovered in 1938, and was immediately confirmed by every laboratory in the world which experimented on it. It took about fifteen years before any commercial use could be made. Cold fusion was confirmed by some laboratories, denied by others, and even the confirmation concerned only the very slight reaction leading to emission of neutrons. This is not the time to jump to a large industrial project.

Yours sincerely,

H. A. Bethe

Hans A. Bethe

HAB:vhr

cc: Congressman James H. Scheuer
Congressman Robert S. Walker, Ranking Minority Member

What to say about cold fusion

Public interest in recent excitements is to be welcomed, especially if it does not turn to anger when attempts to replicate the observation of cold fusion fail — the most probable outcome.

FLEISCHMANN and Pons have done at least one great service for the common cause: they have kindled public curiosity in science to a degree unknown since the Apollo landings on the Moon. In the past few weeks, people thought by those about them to have a few scraps of inside knowledge have been quizzed about the smallest details of the experiments at the University of Utah — those first described in the *Wall Street Journal* and the *Financial Times* on 23 March and at a press conference at Salt Lake City later that same day. The social phenomenon is all the more remarkable because of the wide distribution of those who have a general understanding of nuclear fusion and of the reasons why its practical application would be important.

These impromptu conversations seem generally to be remarkably good-humoured. Those who want to know more, and who in particular ask "Do *you* think it's true?", seem not too much annoyed by the prospect of continued suspense. It is remarkable that so many people are willing to accept that experimental observations, and the inferences drawn from them, acquire validity only by replication. Has what used to be called "the scientific method" now become widely understood?

If so, much of the credit should go to the daily press, which has risen superbly to the challenge of cold fusion. The two financial newspapers that broke the news on 23 March did so in cautious language, making it plain that cold fusion was not then a proven reality, let alone a commercial source of limitless energy. Other daily newspapers have joined in with commendable zeal and sobriety. Inevitably, the US daily press, with its resources and self-discipline, has done best, but newspapers such as the London *Daily Telegraph*, with no great tradition in the field of science reporting, have been magnificent. Reporters have given full and coherent accounts of meetings as far apart as Sicily and Texas, have canvassed opinions with care and have also faithfully reflected the good humour of these hectic weeks.

The good humour may not persist if the experiments cannot be replicated but there are some grounds for optimism. Part of the reason why so many people are so interested seems to be the general delight that a couple of people in widely separated universities have used their own money to pull off a trick on which governments have lavished huge sums of money in the past 30 years, so far without result. So people may well be indulgent if the attempts to replicate cold fusion on the scale described by Fleischmann and Pons prove failures. "It was a brave venture", they may say, "What a pity it did not succeed!"

That would be the best outcome. It is also, of course, possible that the general reaction to the failure of attempts at replication will be more sour. The scientific community's reputation is vulnerable in several respects, not the least of which is that neither the Utah group nor the Brigham Young group (whose account of its work appears on page 737, this issue) had, before seeking publication, carried out the rudimentary control experiment of running their electrolytic cells with ordinary rather than heavy water.

A close friend who is a Soviet biologist, on the telephone from the United States last week, was indignant at this neglect. He is correct. How is this astonishing oversight to be explained to students repeatedly being drilled in the need that control experiments should be as conspicuous in the design of an investigation as those believed to display the phenomenon under study? And how should the neglect be explained to the world at large?

There is no convincing explanation, only extenuating circumstances. Self-imposed secrecy has evidently hampered the investigators, understandably buoyed up by their belief that they had discovered a remarkable new phenomenon and fearful that too much talk about it would give other bigger battalions a chance to steal a march on them. Yet it is unthinkable that, if the authors had felt able from the outset to stand in front of routine laboratory colloquia and give a full account of their work, the question "Have you tried it with ordinary water?" would not have been raised. This glaring lapse from accepted practice is another casualty of people's need to be first with reports of discovery and with the patents that follow.

More subtle doubts perplexing those seeking to replicate the experiments would have been exorcised in the same process. The Brigham Young group, for example, clearly explains how it has been necessary to estimate the number of neutrons emitted from its electrolytic cells by subtracting from its observations of neutrons the background measurements, but Dr John M. Carpenter (one of the referees of the article by Jones *et al.*) explains on page 711 that the background may fluctuate with time, which argues for the need for contemporaneous controls. Sadly, the neutron monitor is a special device, of which there is only one copy....

The Fleischmann and Pons experiments raise bigger questions, if only because the scale of the phenomenon they report is so much greater. Both groups report the detection of neutrons, but the Utah group (*J. Electroanalyt. Chem.* **261**, 301; 1989) requires that there should be 10^{12} fusion reactions a second (within an order of magnitude in either direction) to account for the rate at which heat is produced, while the Brigham Young group is talking of one fusion reaction every 100 seconds or so. But the Utah group has not yet dealt with the natural question whether the observed energy output is energy stored in the palladium electrodes during the preparation of the cell. Records of cell voltage and current for the full duration of each run would go some way to settle the question, but have not been produced.

So are the extenuating circumstances sufficient to avoid the conviction of the scientific community for irresponsibility? No doubt the general opinion will depend on the outcome of attempts at replication, but the community might wish that its reputation did not hang on such a narrow thread, especially because the likelihood of replication fades as the days go by.

So robust scepticism is the only wise view. There may be something in the Brigham Young phenomenon, but that requires careful confirmation. The Utah phenomenon is literally unsupported by the evidence, could be an artefact and, given its improbability, is most likely to be one.

Luckily, there are a few blessings to count. Theoreticians have zealously recalculated the fusion rate between deuterons in a molecule (now reduced by 10 orders of magnitude) and have shown that the fusion rate between protons and deuterons, against naive expectation, is likely to be six orders of magnitude greater still — but still no more than 10^{-54} per molecule of HD per second. A week or so ago, ingenious schemes for bringing bare hydrogen nuclei more closely together in the electron sea of a palladium lattice seemed to offer an escape from scepticism. But not for long, even if it is plain that metal hydrides are an even more interesting field of research than had been thought.

John Maddox

NUCLEAR PHYSICS

The cold fusion family

James S. Cohen and John D. Davies

CLAIMS for the observation of cold fusion are based on two types of evidence. Jones *et al.*, on page 737 of this issue[1], report that they have detected neutrons, with energy characteristic of a fusion reaction, in an electrochemical cell containing deuterium. And Pons and Fleischmann report in the 10 April issue of the *Journal of Electroanalytical Chemistry*[2] the production of unexplained excess heat and also neutrons in a similar cell. It is wise to see whether the observations can be explained using standard physics before seeking novel solutions — especially as the two reports conflict greatly in detail.

Cold fusion is a known process, but not as described in the new work. A muon — a heavy analogue of the electron — substituted for an electron in a deuterium molecule can cause the molecule to contract so that there is a significant probability of the two nuclei coming into contact. A fusion reaction can follow in which energy is carried away by the products. The muon is released and can induce further fusions. This is muon-catalysed fusion[3].

Considerations of this process in the context of the present experiments is natural, because Van Siclen and Jones have previously suggested[4] that very high pressures could similarly induce fusion; because some have suggested that muons from cosmic rays could be responsible for the observed effects; and because lessons about the relative rates of different fusion reactions could be important. The reactions of interest are those of deuterons (d=^{2}H), tritons (t=^{3}H) and protons (p=^{1}H).

$$\mathrm{d + d} + \mu \rightarrow \begin{cases} \mathrm{t(1.01\ MeV) + p(3.02\ MeV)} + \mu & (42\ \text{per cent}) \quad (1a) \\ {}^3\mathrm{He(0.80\ MeV) + n(2.45\ MeV)} + \mu & (58\ \text{per cent}) \quad (1b) \end{cases}$$

$$\mathrm{d + t} + \mu \rightarrow \quad {}^4\mathrm{He(3.5\ MeV) + n(14.1\ MeV)} + \mu \qquad (2)$$

$$\mathrm{p + d} + \mu \rightarrow \begin{cases} {}^3\mathrm{He} + \gamma\,(5.4\ \mathrm{MeV}) + \mu & (85\ \text{per cent}) \quad (3a) \\ {}^3\mathrm{He(0.2\ MeV)} + \mu\,(5.2\ \mathrm{MeV}) & (15\ \text{per cent}) \quad (3b) \end{cases}$$

Here n signifies neutrons; γ, γ-rays; μ, negative muons; the energy carried by fragments is given in megaelectron volts; and the percentages signify branching fractions in muon-catalysed reactions.

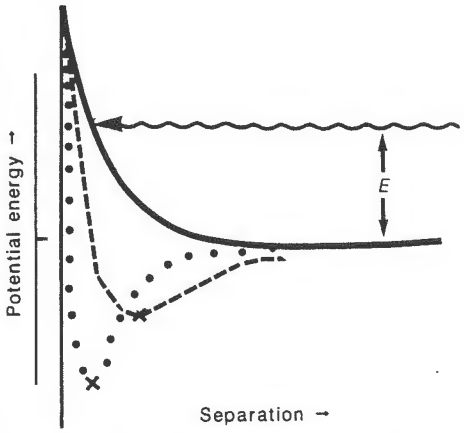
Before fusion, the nuclei are held apart by Coulomb repulsion. This barrier is penetrated by quantum-mechanical tunnelling, rather than being surmounted. Note that tunnelling is controlled more by the width of the barrier than by its height. Thus, replacing the electron in a hydrogen ion by a muon reduces the separation of the nuclei by a factor almost equal to the muon/electron mass ratio (207), reducing the 'tail' of the Coulomb barrier correspondingly. The mean time for a fusion reaction then becomes (ref. 5) 8×10^{-13} s for d–t, 2×10^{-9} s for d–d. (The lesser speed of the d–d reaction arises in part because fusion occurs in a state with one unit of angular momentum which creates a centrifugal barrier to fusion. The d–t molecule is also initially formed in such a state but, because of its asymmetry, rapidly makes a transition to a zero-angular-momentum state.)

It is useful to consider the description of fusion in nuclear-scattering experiments. Besides the probability of barrier penetration, the subsequent interaction between the nuclei (expressed in the 'astrophysical' S function) has an important influence on the fusion rate or cross-section σ:

$$\sigma = \frac{S}{E} \exp\left(-31.3\sqrt{\mu/E}\right)$$

where μ is the 'reduced' mass in atomic mass units of the colliding nuclei and E is the centre-of-mass collision energy in keV. The variation of S for the various reacting nuclei can be crucial. In the low-energy limit, which clearly applies to cold fusion, $S = S_0$ and is smallest for the p–d reaction (2.5×10^{-4} keV barns)[6]: this is because the final stage involves the emission of a γ-ray, an electromagnetic process which is inherently weaker than the strong-force interactions of the other fusion reactions. For d–d, S_0 is 1.08×10^2 (including both channels[7]); for d–t, which involves a strong resonant intermediate state of ^{5}He, S_0 is 1.15×10^4 (ref. 8).

Nevertheless, at low energy the 'Gamow' penetrability factor becomes dominant, and significant variations arise between the various fusion rates because of the variation in reduced mass. Thus for bare nuclei, the d–t fusion rate exceeds that of d–d for collision energies above about 400 eV, and the p–d rate exceeds that of d–d below about 200 eV. The implications of the two factors for the case of cold fusion, in which the Coulomb repulsion is shielded by electron or muon screening, is also instructive. Only the barrier penetrability factor is changed by the screening (see figure); also we have just established some useful energies at which the relative fusion rates change.



Schemetic of the potentials between two nuclei (not to scale): solid line, unscreened; broken, with electron screening (chemical bonding); dotted, with muon screening. To the right of the barrier, electrostatic interactions are dominant; to the far left, strong nuclear interactions occur as expressed by the astrophysical S factor. Wavy line indicates collision energy E in scattering experiments, the classical distance of closest approach (R_0) at the arrow head. The bond length (x) is actually about 207 times shorter for muons than for electrons and the potential well is about 207 times deeper.

From these it is possible to show that muon screening favours d–t fusion; normal electron screening favours p–d fusion (although in this case fusion rates are unmeasurably slow).

The characteristic separation in the isotopic hydrogen molecules is the bond length — normally somewhat greater than the distance R_0 at which repulsion between the nuclei becomes apparent. Allowing the nuclei easily to approach this close is roughly equivalent to colliding the bare nuclei together with a centre-of-mass energy $14/R_0$ eV where R_0 is measured in ångströms. (Such simple considerations have to do for now since an accurate calculation of the penetrability requires knowledge of the potential energy interaction of d–d in the crystal, which is unknown for electrochemical cold fusion.) For the ordinary hydrogen molecule, $R_0 = 0.7$ Å, so that the equivalent energy is about 20 eV, well below the value 400 eV at which d–t fusion becomes faster than d–d. But with the ion's electron substituted by a muon, $R_0 = 4 \times 10^{-3}$ Å, so that the equivalent energy is about 3,500 eV and d–t fusion is clearly favoured (by a factor of 36 in the zero-angular-momentum state according to calculations of C.-Y. Hu (personal communication)). Although muon-catalysed fusion presents the most dramatic evidence of the effect of screening on fusion rates, similar electron-screening effects have been seen in reactions such as (ref. 9) ^{3}He + d → ^{4}He + p.

Screening effects should be considered in discussing the new results. Any means of reducing R_0, vibrational excitation of the bond, a reduction in the bond length, or even a flattening of the potential-

energy well could be effective. It is interesting to note that for the shielding distance relevant to the new studies (of the order of 10^{-1} Å) the p–d fusion rate should exceed that for d–d. This reaction should be studied experimentally; there seems to be no advantage in using d–t, however.

Some have suggested that the new results could be an artefact due to fusion catalysed by cosmic-ray muons. This seems most improbable: the muon stopping flux is not high; the fraction captured by deuterium is low; and the fraction retained by deuterium is even lower. Also, because the muonic molecule formation rate is too slow and because a significant fraction of the muons stick to the helium at fusion, each one can catalyse only a few reactions.

It is reasonable to take 0.1 g^{-1} h^{-1} to be the upper limit to the rate for muons being stopped in a Utah basement (J. Osborne, personal communication). Heavy elements are better at stopping muons, the capture ratio being roughly proportional to nuclear charge Z (ref. 10). Moreover, any dμ would initially have about an electron volt of kinetic energy[11]: while slowing down, many muons would be transferred to the high-Z atoms[10] (high-Z contaminants are kept to less than 10^{-4} of contents in muon-catalysed fusion experiments). Lastly, the time for formation of the muonic deuterium molecule is comparable to the muon lifetime (2.16 μs) so that few fusions can be catalysed by each muon.

For the sake of argument, however, assume that the above limitations can be removed: that is suppose that channelling in the crystalline electrodes keeps muons from the high-Z atoms and the deuterium density is so high that d–d collision rates do not limit the process. The lifetime of a muon would then be sufficient to catalyse 1,000 fusions. But this also assumes it is continuously available for catalysis. In fact, in 8 per cent of d–d fusions the muon sticks to the ^{3}He to form muonic helium [12]. Although in subsequent collisions of the muonic helium with deuterium, the muon could be stripped this will happen for only 32 per cent of captured muons, even with very high deuterium density[13]. (The high-Z elements have large stripping cross sections, but stripping in this case often results in capture of the muon and so is not helpful[14].) Hence, even under the most favourable circumstances, a single muon would catalyse only about 20 d–d fusions (or 340 d–t fusions). With the above muon stopping rate and assuming an electrode mass of 3 g, the neutron production rate would be 2×10^{-3} s^{-1}, well short of the 0.41 s^{-1} reported by Jones *et al.*[1].

Of course, if the fusion is catalysed by some stable charged quasiparticle in the electrode, one no longer has to contend with the short lifetime encountered with the muon. This can even offset the slower fusion rate that is inevitable with quasiparticle masses less than that of the muon. Van Siclen and Jones's earlier calculations[4] suggest that reducing the hydrogen bond length by a factor of 2 (we believe a larger factor is needed), achievable with a quasiparticle with twice the electron mass, might give the neutron production rate reported now by Jones *et al.*[1]. Large effective electronic masses are often associated with transition metal crystals like palladium; unfortunately, the notion of heavy electrons is related to the nonlocal interactions of electrons with the crystal lattice; it is not easy to see how the local concept of tight binding can be squared with this.

The neutron rates observed by Fleischman and Pons[2] present further difficulties. Not only are they much higher, at about 10^4 s^{-1}, than those of Jones *et al.*, but they are a factor of about 10^9 less than expected from the rate of d–d fusions necessary to generate the heat they report. Thus they suggest some form of aneutronic fusion is occuring: for example a preference for reaction 1*a* over 1*b*.

The equality of rates for 1*a* and 1*b* given by charge symmetry of nuclear forces is slightly altered by Coulomb 'isospin' mixing (G. M. Hale, personal communication). For bare nuclei (plasma fusion) the p + t product channel is very slightly favoured, but for muon-catalysed fusion the n+^{3}He channel is favoured by a ratio of 1.4 (ref. 15). The latter is a consequence of fusion taking place in a state of unit angular momentum. In any event, it would seem quite difficult to suppress the neutron channel or to hide the tritium from assay. The normally rare radiative channel, d + d → ^{4}He + γ (20 MeV), would also show up in the lower-energy γ-ray detector from pair production. On the other hand, only heat would be detected if it were somehow possible for the energy to go directly into the palladium crystal lattice ('Mössbauer fusion'?), but such a process is expected to be strongly suppressed by the vastly different energy scales of the crystal lattice (eV) and fusion

Fusion in 1926: *plus ça change*

ON September 17, the *Morning Post* published a Reuter message from Berlin to the effect that Profs. Paneth and Peters of that city had, after years of experimenting, succeeded in transforming hydrogen into helium "with the aid of particles of metal." This announcement, if correct, is of great importance and will evoke even more interest than the claim by Miethe and Stammreich to have transmuted mercury into gold.

No particulars are yet to hand concerning the methods adopted by Profs. Paneth and Peters, for the statement "with the aid of particles of metal" is meaningless as it stands. The experimental difficulties must be very great, not only in obtaining the energy necessary for such a change, but also in applying it under the appropriate conditions. Moreover, helium is an atmospheric gas, and traces of it are difficult to eliminate by the methods of evacuation and adsorption at present in use; belief or disbelief in the Reuter message must be reserved pending further and more definite evidence.

* * *

PROFS. F. Paneth and K. Peters describe in outline how they have succeeded in detecting the presence of very minute amounts of helium, of the order of one hundred millionth of a cubic centimetre, derived from hydrogen which had been absorbed by finely divided palladium at the ordinary temperature.

Finely-divided palladium, either as sponge, 'block' or palladinised asbestos, was used to absorb hydrogen at room temperature. The residual gas obtained after a 12-hours' contact between palladium and hydrogen exhibited four or five lines of the helium spectrum; there was also a distinct proportionality between the amount of helium observed and the duration of the time of contact. The activity of the different palladium preparations employed varied considerably; it invariably diminished with repeated use. No helium production was observed with palladium preparations that did not absorb hydrogen. They were not able to detect any trace of the energy liberated during the transformation, and they point out that the amount set free from the conversion of such small quantities of hydrogen — about 0.28 calorie — would be extremely difficult to detect, particularly if thermal changes due to absorption or formation of compounds also take place. They incline to the view that the liberated energy is more likely to appear as radiation, γ or Millikan-rays, than as heat.

* * *

A FEW months ago, K. Peters and I published an account of experiments we had made in an attempt to transmute hydrogen into helium (*Ber. d. Deutsches Chem. Ges.*, 59, 2039, 1926). As a result of further experiments, we are in a position to give an explanation of the occurrence of the observed very small quantities of helium in our experiments without having recourse to the assumption of a synthesis of helium.

In the communication we discussed the possibility of regarding the helium dissolved in the glass as an explanation of the observed effects, but blank experiments led us to the conclusion that the quantity of helium liberated in this way was beyond the sensitivity of our method of detection. In the interval we have carried out experiments that show that the liberation of helium from glass is dependent on the presence of hydrogen. Thus glass tubes which gave off no detectable helium when heated in a vacuum or in oxygen yield helium in quantities of the order of 10^{-9} c.c. when heated in an atmosphere of hydrogen.

As a result we have thus established that, in using an apparatus made of glass, one cannot make any trustworthy statement as to the origin of 10^{-9} c.c. of helium if air comes in contact with the apparatus.

From *Nature* **118**, 455 & 556 (1926); **119**, 706 (1927).

(MeV). Likewise, as indicated by the small branching fraction for reaction (3*b*), transfer of all the energy to a heavy electronic quasiparticle, even if it exists, is highly unlikely unless it is even more massive than the muon. Curiously, an excess amount of ^{4}He did show up in the assay done by Fleischmann and Pons.

The above difficulties in interpreting the Fleischmann and Pons experiment in terms of d–d fusion lead one to consider the possibility of other nuclear reactions that might not produce neutrons or tritons. The electrolyte contained lithium so that $^6Li + d \rightarrow {}^4He + {}^4He$ fusion is possible at the electrode surface or with long lithium drift times into the palladium; the intermediate state, ^{8}Be has a rich set of resonant energy levels to enhance the reaction, but it is difficult to see how the neutronic reactions $^6Li + d \rightarrow {}^7Be + n$ and $^7Li + d \rightarrow n + {}^4He + {}^4He$ could be suppressed. Another difficulty is the smaller penetrability that comes from the greater nuclear charge, the higher reduced mass and the larger bond distance. For similar reasons, a cold fusion of d with Pd can be considered highly unlikely. Here again we note that there is the possibility of reaction with p as well as d, namely $^7Li + p \rightarrow \alpha + \alpha$.

In all this discussion, we have presumed that the current observations[1,2] truly belong to the cold fusion family. We might point out that they could be but a distant cousin. For example, experiments show that d–d fusion neutron emission can accompany the fracture of a LiD crystal[16]; the interpretation is that deuterons are accelerated to kiloelectron volt energies by the strong electric fields at the propagating crack. Such an effect might occur in the embrittled Pd or Ti crystal, in which case we would have 'microscopically hot' fusion under very unusual conditions. Neutrons would be emitted in bursts, not continuously. Other means of creating cracks suddenly would lead to similar effects. □

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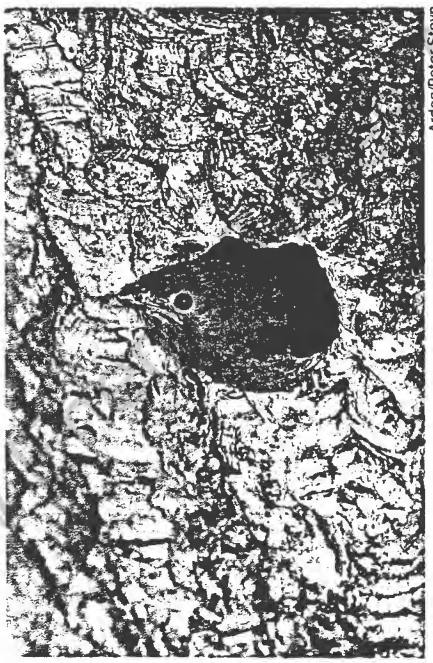
ECOLOGY

Honeyguides and humans

Robert M. May

One of my favourite children's books is an engagingly illustrated account of some of the mutualistic associations found in nature. One such illustration is of the honeyguide — appropriately named *Indicator indicator* — perched on a branch over a bee colony being torn apart by a badger-like ratel. The ratel has been led to the hive by the bird, and after the ratel has finished feasting on the honey, the honeyguide will feed on fragments of honeycomb left behind. The honeyguide needs the ratel because the bee colonies are typically situated in large trees, rock crevices or termite mounds in such a way as to be inaccessible to the unaided bird; the ratel apparently benefits from the honeyguide's deliberately leading it to the source of honey. Isack and Reyer[1] have just completed a 3-year study in which they document the involvement of a third species in this association — humans.



Young honeyguide leaving the nest.

Rock paintings in the Sahara, Zimbabwe and South Africa show that humans have collected honey in Africa for at least 20,000 years[2]. Many anecdotes (the earliest dating back to the seventeenth century[2]) suggest that humans have cut into the honeyguide–ratel dance, using the bird to help guide them to honey sources. Sceptics have viewed these anecdotes as romantic myths, and the issue may soon become moot because in many areas honey is increasingly obtained from bee-keeping or is being replaced by commercial sugar or other products; in these areas, the birds no longer guide.

Isack and Reyer's study[1] of the interactions between honeyguides and nomadic Boran people in northern Kenya is therefore timely and interesting. Treating this system as if it were any other field study of a mutualistic association, the authors show that, in unfamiliar areas, honey-hunting human groups take on average 3.2 hours to find each bees' nest when guided by a bird, and 8.9 hours when not guided. This roughly threefold reduction in searching efficiency without a honeyguide is a conservative estimate, because the figures do not include the many days of unguided search on which no nest was found.

Isack and Reyer also document the benefit to the birds: 96 per cent of the nests they saw discovered (178 of 186) would not have been accessible to the birds until humans had opened them with tools. In addition, the Boran's use of smoky fire reduces the bird's risk of being stung. Because of the pronounced benefits to both parties, it is not surprising that humans and honeyguides have elaborated upon the previous ratel–honeyguide association by developing their own interspecific communication system. Humans attract the bird with a penetrating whistle that can be heard more than 1 km away; Isack and Reyer found such whistles doubled the rate at which birds were encountered. On its part, the honeyguide attracts human attention by flying close or hopping around among perches, emitting a characteristic double-noted call.

Once humans and birds are engaged in a cooperative quest, Isack and Reyer show that the bird leads in consistently direct routes to colonies up to 1 km or more distant. Isack and Reyer find quantitative support for Boran honey-collector lore, demonstrating that three measures of bird behaviour decrease with diminishing distance to the bee colony: (1) the length of time the bird disappears after the first encounter; (2) the distance between the perches where the bird waits until the follower has caught up; and (3) the height of such perches. On arrival at the nest, the bird perches close to it and gives a characteristic 'indication' call (this call, like the initial one, is documented in sonograms). The bird also hops among close perches, often circling the nest in between perching. Isack and Reyer remark that this behaviour of effectively reducing the 'step length' as the goal of the search is approached is another example of a pattern (often called area-restricted search) that is ubiquitous in nature, being found, for instance, in flies looking for sugar particles[3], parasitoids searching for hosts to oviposit on or in[3], or schistosome miracidia homing in on snails as intermediate hosts[4]. ▶

Cold fusion: what's going on?

SIR—A significant point which is not widely known, and may therefore be overlooked in neutron measurements of cold fusion rates, is the possibility of contamination by cosmic-ray-generated neutrons; these should be taken into account in the design and interpretation of experiments.

The cosmic-ray-induced neutron background arises primarily from extra-solar protons with energies above a few GeV, which can penetrate the Earth's atmosphere and the Sun's and Earth's magnetic fields[1]. Primaries and secondaries reaching the surface include neutrons and other energetic particles which produce neutrons in the atmosphere and the first few metres of the surface by spallation reactions. While the spectrum contains neutrons up to energies comparable to incident particle energies, a major component is due to evaporation of neutrons from struck nuclei; at birth these have energies in the range 1–3 MeV, and appear as a knee or shoulder on an otherwise continuous energy distribution. There is also a substantial component consisting of 'thermal' neutrons, which have slowed down in the environment to a poorly equilibrated thermal distribution below energies of about 0.1 eV as well as 'epithermal' neutrons whose distribution is roughly inversely proportional to the energy in the range 1 eV to 1 MeV. At energies above a few MeV, the spectrum tails off rapidly; this cascade component contains about 10% of the total. The flux of each of the low-energy components is of the order of 10^{-2} neutrons cm^{-2} s^{-1} at sea level and middle latitudes.

These figures vary according to altitude (about twice as great at 1,500 m elevation), and from time to time, mostly because of variations in atmospheric density and solar and geomagnetic field intensity. The e-folding thickness in the atmosphere is about 150 g cm^{-2}, so that, for example, barometric pressure variations of ±13 mm of mercury cause about ±10% flux variations. Sometimes, when dealing with such a general source of neutrons, material intended as shielding, and even detector material itself, can act as a source, so that some care in this respect is called for in the measurements.

As it happens, the counting rates due to cosmic-ray-induced neutrons are of the same order of magnitude as the counting rates observed in the neutron and secondary radiation detectors in many of the measurements being made. And in detectors that disperse the spectrum, the evaporation peak in the energy distribution due to cosmic-ray-induced neutrons is at nearly the same energy as that expected from deuteron–deuteron fusion, 2.45 MeV. These observations, coupled with the (admittedly weak, ±10%) temporal (hourly, daily) variation of the cosmic-ray-induced neutron fluxes require that this background be carefully accounted for.

Comparable neutron fluxes can be generated by accelerators, isotope sources and nuclear reactors, even at considerable distances; these contaminants of neutron measurements must also be reckoned with. Obvious means for suppressing these backgrounds are time-gating of the source, monitoring spurious sources with a second detector operated simultaneously with the detector(s) near the source under investigation, or going underground — 350 g cm^{-2} (two or three metres of earth or concrete on all sides) should reduce the cosmic-ray neutrons by a factor of about 10.

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Dr Carpenter, a referee of the paper by Jones *et al.* on page 737, provided this comment at our invitation.

The following are extracts from the substantial numbers of letters from readers offering explanations of the two series of cold fusion experiments which have been generally reported.
Editor, *Nature*.

SIR—From the newspaper accounts, the very small flux of neutrons generated during the experiment of Fleischmann and Pons is being taken as proof that their conclusion is not valid, and that nuclear reactions between deuterons do not occur under the conditions they describe.

But when the kinetic energy is as small as in their experiment, the neutron and proton components of the deuteron do not behave in the same way, because the nucleus of the target atom repels the proton but not the neutron. Thus, the neutron can be captured by the target nucleus while the proton, which remains outside the Coulomb barrier, will fly off.

This process, first recognized by Oppenheimer and Phillips[1] in 1935 leads to a pure (d, p) reaction and has a relatively high probability of occurrence, certainly much greater than that of the (d, n) reaction[2].

It follows that if the experiments described really brought the deuterium nuclei close enough together to interact, one should expect no neutron emission and a reaction rate much higher than that evaluated on the basis of the high-energy model.

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SIR—The only recognized mechanism for nuclear fusion at ambient temperature is that induced by negative muon binding of precursor hydrogen isotope molecules as first described by Frank[1] with estimates of fusion rates by Sakharov[2] and experimental observations by Alvarez[3] some 30 years ago. It is now known that more than 100 fusion events may be induced by a single muon during its lifetime of 2.2×10^{-6} seconds in a mixture of liquid deuterium and tritium[4]. If this number could be increased by a factor of 1,000, a break-even fusion reactor could be the result[5].

It is tempting to interpret the recent claims in terms of this process. It should be remembered that more than 70 per cent of the cosmic-ray flux at the Earth's surface consists of positive and negative muons. There are about 200 m^{-2} s^{-1} with a stopping rate in an absorber of some 2×10^{-5} g^{-1} s^{-1}. The flux may be twice as great at the altitude of Salt Lake City[6], which would be equivalent to more than two muons a minute in an absorbing volume of about a litre.

The rate at which fusion events could occur is limited by the rate of formation of muon-bound molecules, which is itself a sensitive function of parameters on the atomic scale, whence its dependence on resonance effects[7] and temperature[8]. Once a muon is captured, the resulting muonic molecule is two orders of magnitude smaller than the typical lattice spacing in solids, so that free diffusion may be expected.

The fact remains that muon-induced fusion has not yet been reported in metallic compounds of hydrogen isotopes, and indeed has been considered unlikely because of the preferential capture of muons by the heavier metal nuclei. On the hypothesis that this loss mechanism is suppressed by a resonance or band structure effect in deuterium-loaded palladium, it is possible to estimate the turnover number required to explain the effects which have been reported.

Jones *et al.*[9] observe a neutron count-rate of 4×10^{-3} s^{-1} with a neutron detection efficiency of about 1% in a volume of 160 ml. If the neutrons observed are products of the reaction, in a muon-deuterium molecule, of two deuterons to yield ^{3}He and a 2.45-MeV neutron, one should also allow for the equally probable reaction yielding ^{3}H and a proton, which would not have been detected by the neutron monitor. This implies five fusion events per second per litre. The required turnover number is then of the order of 100, comparable with that already known for the case of a mixture of liquid isotopes, but significantly greater than that in pure

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deuterium.

The consequence is that fusion induced by cosmic-ray muons cannot be excluded as an explanation of the reports of radiation effects in palladium loaded electrochemically with deuterium, although the estimates of the rate of muon-induced fusion is much less than that required by the thermal observations of Fleischmann and Pons[10].

These developments emphasize the need for experimental data on the effects of negative muons in solids, especially metal deuterides and tritides, which are at present lacking in the open literature. Arrangements are in hand to investigate the reported palladium-deuterium effects with a muon source of greater flux than that of the natural cosmic ray radiation.

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Sir—In my theoretical investigations of the electronic structure of the H_2^+ molecule (*Phys. Lett.* **123**, 170; 1987), I have found that the two nuclei and the electron can form a collapsing quasi-molecule — a compound system whose dimensions decrease with time to zero. (The extreme case is when the electron is at the centre-point between the two nuclei.) In general, in collapsing molecules like these, the repulsive Coulomb interaction of the nuclei and the gas-kinetic pressure of the electron are less than the attractive Coulomb forces between the two nuclei and the electron. The closest approach of the two nuclei depends on the initial state of the electron, its binding energy and the mean value of the kinetic energy in particular.

The probability of the tunnelling effect is therefore identical with the probability of formation of a collapsing quasi-molecule. Thus it is clear that the electrons present in the matter are responsible for the Coulomb-barrier tunnelling, and that the process which has been observed depends on quasi-molecular systems.

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Sir—Reports of the experiments by Fleischmann and Pons contain a paradox — that, if fusion reactions do occur in them, either too much energy is liberated or too few neutrons are detected. I wish to suggest a possible explanation.

I start from the hypothesis that the palladium contains regions where the density of deuterons is sufficiently great for d-d fusion to occur by one of the reactions leading either to ^{3}H and a proton or to ^{3}He and a neutron. The product particles will be produced within a region where the density of other particles is very great. The mean free paths of the particles will then be very small, and it appears reasonable to assume that even though these high-density regions will be geometrically small, they will be so optically large that even the most penetrating particles, such as neutrons, will remain trapped inside them. In this situation, the particles produced by fusion reactions will undergo multiple scattering collisions until a new reaction occurs.

Several such reactions are possible, including fusion reactions of deuterons with ^{3}H and ^{3}He (yielding ^{4}He and a neutron or proton respectively) and the radiative capture of protons or neutrons by deuterons. These reactions are exothermic, releasing large amounts of energy.

It is crucial that these processes can also form multiplicative chains, especially if the γ-ray photons released by radiative capture reactions yield further energetic neutrons and protons by the photodisintegration of deuterons. The reaction chains will come to an end only when reactive particles escape from high-density regions to those where the density is insufficient to sustain them.

The main products of these reaction chains will be α-particles, but the reactive particles such as neutrons and ^{3}H will only infrequently be released to the environment.

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Pulsar formation

Sir—Lindley[1] states that the report[2,3] of a half-millisecond pulsar in the remnant of supernova 1987A "has surprised everybody and could, if confirmed, stand theory on its head". He has apparently overlooked several papers (refs 4 and 5, for example) suggesting that pulsars may form directly as rapidly rotating neutron stars "in original spin" with weak magnetic fields, without the need for "resurrection" after birth as a slow rotator followed by subsequent decay of the magnetic field and spin-up by accretion of matter from a companion star[6].

Even before the discovery of millisecond pulsars, we discussed[7] the question of why collapsed stars rotate so slowly, and pointed out that if there was significant mass ejection during the formation of a neutron star, and if it had a strong magnetic field, it was likely to be born spinning slowly. We argued, furthermore, that if the progenitor giant core of the neutron star has a strong magnetic field, it is likely to have been rotating relatively slowly, as the magnetic field would have enhanced transfer of angular momentum from it to the envelope during earlier evolutionary phases. We concluded that if weak-field neutron stars could form at all, they would be born spinning fast. If the optical emission from the half-millisecond pulsar arises from incoherent synchrotron radiation at the light cylinder, and scaling by the optical luminosity and magnetic field of the Crab pulsar, it follows that the half-millisecond pulsar indeed has a weak magnetic field of $B \sim 10^8$ gauss.

With the discovery of six millisecond pulsars (with periods $\leq$12 ms), of which four are in binary star systems, several authors concluded that 'resurrection' was required to account for their observed properties. A key requirement for the 'resurrection' model is that the magnetic field of neutron stars must decay on a timescale of a few million years. But arguments against significant neutron-star magnetic-field decay have been proposed (for example, ref. 8). Futhermore, calculations by Sang and Chanmugam[9] showed that there are serious difficulties with all models so far proposed for field decay. In addition, there is no satisfactory detailed model that explains how single millisecond pulsars can be formed from binary star systems. Even in the case of PSR1957 + 20, where there is evidence of matter evaporating from the companion, the spin-down rate indicates insufficient energy loss from the pulsar to evaporate the entire companion star. When combined with all the difficulties of the 'resurrection' model, the discovery of the half-millisecond pulsar seems to provide substantial further support for the view that at least some millisecond pulsars can be born "in original spin".

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Observation of cold nuclear fusion in condensed matter

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When a current is passed through palladium or titanium electrodes immersed in an electrolyte of deuterated water and various metal salts, a small but significant flux of neutrons is detected. Fusion of deuterons within the metal lattice may be the explanation.

FUSION of the nuclei of isotopes of hydrogen is the principal means of energy production in the high-temperature interiors of stars. In relatively cold terrestrial conditions, the nuclei are surrounded by electrons and can approach one another no more closely than is allowed by the molecular Coulomb barrier. The rate of nuclear fusion in molecular hydrogen is then governed by quantum-mechanical tunnelling through that barrier, or equivalently, the probability of finding the two nuclei at zero separation. In a deuterium molecule, where the equilibrium separation between deuterons (d) is 0.74 Å, the d-d fusion rate is exceedingly slow, about 10^{-74} per D_2 molecule per second[1].

By replacing the electron in a hydrogen molecular ion with a more massive charged particle, the fusion rate is greatly increased. In muon-catalysed fusion, the internuclear separation is reduced by a factor of ~200 (the ratio of the muon to electron mass), and the nuclear fusion rate correspondingly increases by about eighty orders of magnitude. Muon-catalysed fusion has been shown to be an effective means of rapidly inducing fusion reactions in low-temperature mixtures of hydrogen isotopes[2,3].

A hypothetical quasi-particle a few times as massive as the electron would increase the cold fusion rate to readily measurable levels of $\sim 10^{-20}$ fusions per d-d molecule per second[1]. The results reported here imply that a comparable distortion of the internuclear wavefunction can be realized when hydrogen isotope nuclei are loaded into metals under certain conditions. We have discovered a means of inducing nuclear fusion without the use of either high temperatures or radioactive muons.

Indirect evidence

Observations of naturally occurring ^{3}He in the Earth suggested to us new directions for laboratory investigations of nuclear fusion in condensed matter. ^{3}He is produced by the following fusion reactions:

$$p + d \rightarrow {}^3He + \gamma(5.4\ MeV) \quad (1)$$

$$d + d \rightarrow {}^3He(0.82\ MeV) + n(2.45\ MeV) \quad (2a)$$

$$\rightarrow t(1.01\ MeV) + p(3.02\ MeV) \quad (2b)$$

Tritium (t) decays with a 12.4-yr half-life to produce ^{3}He. The well established high ^{3}He/^{4}He ratio in solids, liquids and gases associated with volcanoes and other areas of high heat flow[4-6] suggests fusion as a possible source for the ^{3}He.

To estimate a possible rate of fusion in the Earth, we assume a simple, steady-state model in which the known flux of ^{3}He out of the mantle, 2×10^{19} ^{3}He atoms per second[7], arises from p-d fusion occurring uniformly in the mantle water reservoir, taken as $\sim 1.4\times10^{24}$ g (R. Poreda, personal communication). Note that if the Earth contains 'primordial' ^{3}He, our calculated rate will be an upper limit; on the other hand, if fusion-produced ^{3}He is stored in the mantle (so that the outward flux does not equal the production rate), our value will be a lower limit. As each p-d fusion produces one ^{3}He atom, and as the isotopic abundance of deuterium in water is $\sim 1.5\times10^{-4}$ deuterons per proton, we infer a geological fusion rate constant, λ_f, of

$$\lambda_f \approx \frac{2\times10^{19}\ {}^3He\ atoms\ s^{-1}}{1.4\times10^{43}\ deuterons}$$

$$\approx 10^{-24}\ fusions\ d^{-1}\ s^{-1} \quad (3)$$

This rate is fifty orders of magnitude larger than that expected in an isolated HD molecule, and fusion at this rate could be detected if reproduced in the laboratory.

Cold nuclear fusion may be important in celestial bodies other than the Earth. Jupiter, for example, radiates about twice as much heat as it receives from the Sun. It is interesting to consider whether cold nuclear fusion in the core of Jupiter, which is probably metallic hydrogen plus iron silicate, could account for its excess heat. Heat is radiated at an approximate rate of 10^{18} watts, which could be produced by p-d fusions occurring at a rate of 10^{30} s^{-1}[1]. Assuming a core of radius 4.6×10^9 cm, containing mostly hydrogen, with density ~ 10 g cm^{-3} and a deuteron/proton ratio of $\sim 10^{-4}$, we deduce a required p-d fusion rate of $\lambda_f \approx 10^{-19}$ fusions $d^{-1}\ s^{-1}$ if all the heat derives from fusion. Catalysed nuclear fusion at this rate could be readily measured in the laboratory.

Further evidence for cold nuclear fusion in condensed matter comes from studies of ^{3}He and ^{4}He in metals. There have been several reports of high ^{3}He concentrations in metal crucibles and foils (H. Craig, R. Poreda, A. Nier, personal communications), consistent with *in situ* formation by cold fusion. In particular, Mamyrin *et al.*[8] report the occurrence of patchy, high concentrations of ^{3}He in a number of metal foils. Electrolytic refining of the metals could have provided the appropriate conditions for the cold nuclear fusion reactions (1) and possibly (2). Among several possible explanations for the observations, the authors suggest an analogue of muon catalysis[8].

Detection of cold-fusion neutrons

The considerations outlined above led to laboratory experiments performed at Brigham Young University to determine whether cold nuclear fusion can actually occur in condensed matter. We now report the observation of deuteron–deuteron fusion at room temperature during low-voltage electrolytic infusion of deuterons into metallic titanium or palladium electrodes. The fusion reaction (2*a*) is apparently catalysed by the deposition of d^+ and metal ions from the electrolyte at (and into) the negative electrode. Neutrons with an energy of ~2.5 MeV are clearly detected with a sensitive neutron spectrometer. The experimental layout is shown in Fig. 1.

The neutron spectrometer, developed at Brigham Young University over the past few years (ref. 9 and manuscript in preparation) has been crucial to the identification of this cold fusion process. The detector consists of a liquid organic scintillator (BC-505) contained in a glass cylinder 12.5 cm in diameter, in

which three glass scintillator plates doped with lithium-6 are embedded. Neutrons deposit energy in the liquid scintillator through multiple collisions, and the resulting light output yields energy information. As their energy decreases, the neutrons are scavenged by ^{6}Li nuclei, and the reaction $n + {}^6Li \rightarrow t + {}^4He$ results in scintillations in the glass. Pulse shapes and amplitudes from the two scintillators differ; the two distinct signals are registered by two photomultiplier tubes, whose signals are summed. A coincidence of identified signals from the two media within 20 μs identifies an incoming neutron that has stopped in the detector.

The spectrometer was calibrated using 2.9- and 5.2-MeV neutrons generated by deuteron–deuteron interactions at 90° and 0°, respectively, with respect to a deuteron beam from a Van de Graaff accelerator. The observed energy spectra show broad structures which imply that 2.45-MeV neutrons should appear in the multichannel analyser spectrum in channels 45–150. The stability of the detector system was checked between data runs by measuring the counting rate for fission neutrons from a broad-spectrum californium-252 source.

We have performed extensive tests to verify that the neutron spectrometer does not respond preferentially in this pulse height range to other sources of radiation such as thermal neutrons. In particular, we made unsuccessful efforts to generate false 2.5-MeV neutron 'signals' by using various γ-ray and neutron sources and by turning auxiliary equipment on and off. Neutron-producing machines such as the Van de Graaff accelerators were off during all foreground and background runs.

Many background runs were made using operating cells (described below) containing standard electrodes and electrolytes, except that H_2O replaced the D_2O; other background runs were made using both new and previously used standard cells containing D_2O plus the usual electrolyte but with no electrical current. The individual background runs were all featureless and closely followed the pattern of the integrated background shown in Fig. 2. Background rates in the neutron counter are $\sim 10^{-3}\ s^{-1}$ in the energy region where 2.5-MeV neutrons are anticipated. By comparing energy spectra from γ-ray and neutron sources we have determined that approximately one-fourth of the observed background events arise from accidental coincidences of γ-rays and three-fourths from ambient neutrons. The γ-ray background comes mainly from radioactive radium and potassium in the surrounding materials. We attribute the ambient neutrons to cosmic-ray sources. Although the typical neutron evaporation spectrum (at birth) has a broad maximum near 2.5 MeV (ref. 10), Monte Carlo calculations show that moderation in the source medium (predominantly the shielding surrounding the detector) will wash out this structure and produce a smoothly decreasing background spectrum above 0.5 MeV, as observed.

The predicted and measured absence of structure in the spectrum of cosmic-ray-produced neutrons will not be influenced by the relatively small temporal variations that may occur in the cosmic-ray flux, such as the observed decreases that may accompany solar flares. This means that the observed peak at 2.5 MeV cannot be accounted for by ambient-neutron background variations, because, as explained below, the analysis is based on the shape of the spectra and not simply on rates. Low-energy cosmic-ray muons would be rapidly scavenged by nuclei with high atomic number, so as to reduce muon-catalysed d–d fusion to a negligible level[2,3]. Considering volume and solid angle, the rate of production of neutrons by muons absorbed by carbon nuclei in the detector exceeds that from muons absorbed by oxygen nuclei in the electrolytic cells by a factor of ~60. Thus, the presence or absence of electrolytic cells is an unimportant perturbation in the background.

During the search for suitable catalytic materials, the following (unoptimized) prescription for the electrolytic cells evolved. It began with salts typical of volcanic hot springs and included electrode-metal ions. The electrolyte is typically a mixture of ~160 g D_2O plus various metal salts in ~0.1 g amounts each: $FeSO_4 \cdot 7H_2O$, $NiCl_2 \cdot 6H_2O$, $PdCl_2$, $CaCO_3$, $Li_2SO_4 \cdot H_2O$, $Na_2SO_4 \cdot 10H_2O$, $CaH_4(PO_4)_2 \cdot H_2O$, $TiOSO_4 \cdot H_2SO_4 \cdot 8H_2O$, and a very small amount of AuCN. The pH is adjusted to ≤3 with HNO_3. All 14 runs reported here began with this basic electrolyte.

Titanium and palladium, initially selected because of their large capacities for holding hydrogen and forming hydrides, were found to be effective negative electrodes. Individual electrodes consisted of ~1 g purified 'fused' titanium in pellet form, or 0.05 g of 0.025-mm-thick palladium foils, or 5 g of mossy palladium. Typically 4–8 cells were used simultaneously. The palladium pieces were sometimes re-used after cleaning and roughening the surfaces with dilute acid or abrasives. Hydrogen bubbles were observed to form on the Pd foils only after several minutes of electrolysis, suggesting the rapid absorption of

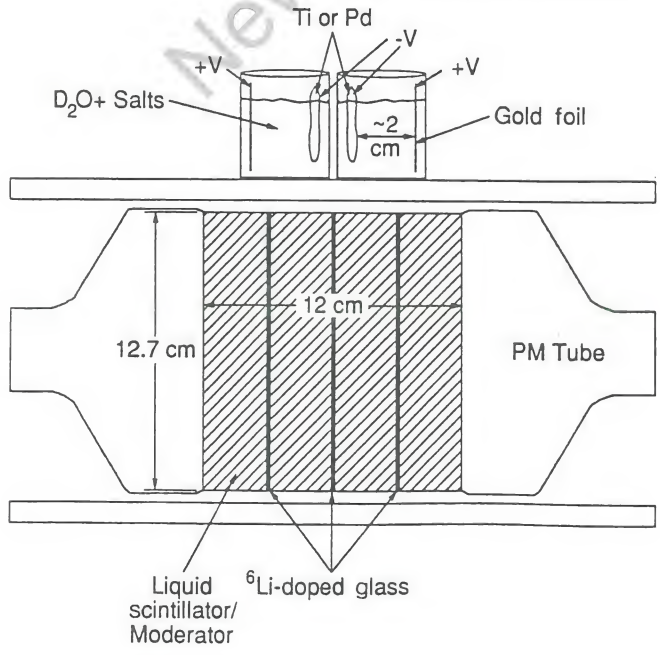

FIG. 1 Schematic diagram of the experiment. Electrolytic cells are shown on top of the neutron spectrometer.

deuterons into the foil; oxygen bubbles formed at the anode immediately. Gold foil was used for the positive electrodes. Direct-current power supplies provided 3–25 volts across each cell at currents of 10–500 mA. Correlations between fusion yield and voltage, current density, or surface characteristics of the metallic cathode have not yet been established.

Small jars, ~4 cm high and 4 cm in diameter, held ~20 ml of electrolyte solution each. The electrolytic cells were placed on or alongside the neutron counter, as shown in Fig. 1. The present cells are simple and undoubtedly far from optimum. Nevertheless, the present combination of our cells with the neutron spectrometer is sufficient to establish the phenomenon of cold nuclear fusion during electrolytic infusion of deuterium into metals.

Figure 2 shows the energy spectrum obtained under the conditions described above, juxtaposed with the (scaled) background spectrum. We acquired about twice as much background data as foreground data. Assuming conservatively that all deviations from background are statistical fluctuations, we scale the background counts by a factor of 0.46 to match the total number of foreground counts over the entire energy range shown in Fig. 2. A feature in channels 45–150 rises above background by nearly four standard deviations. This implies that our assumption is too conservative and that this structure represents a real physical effect. After re-scaling the background by a factor of 0.44 to match the foreground levels in regions just below and just above this feature, the difference plot (Fig. 3) is obtained. It shows a robust signal centred near channel 100, with a statistical significance of almost five standard deviations. A gaussian fit to this peak yields a centroid at channel 101 with a standard deviation of 28 channels, and an amplitude of 23.2 ± 4.5 counts. Both the position and width of this feature correspond to those expected for 2.5-MeV neutrons, according to the spectrometer calibration. The fact that a significant signal appears above background with the correct energy for d–d fusion neutrons (~2.5 MeV) provides strong evidence that room-temperature nuclear fusion is occurring at a low rate in the electrolytic catalysis cells.

Fusion rate determination

It is instructive to examine the fourteen individual runs which enter into the combined data discussed above. These runs were performed over the period 31 December 1988 to 6 March 1989. Figure 4 displays, for each run, the ratio of foreground count rate in the 2.5-MeV energy region to the background rate

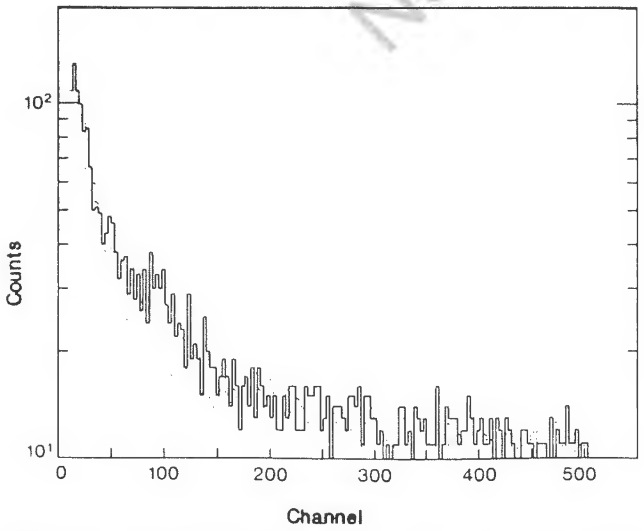

FIG. 2 Foreground (solid) and background (dashed) counts as a function of pulse height (corresponding to neutron energy) in the neutron spectrometer. Ten counts have been added to each three-channel bin for clarity of presentation.

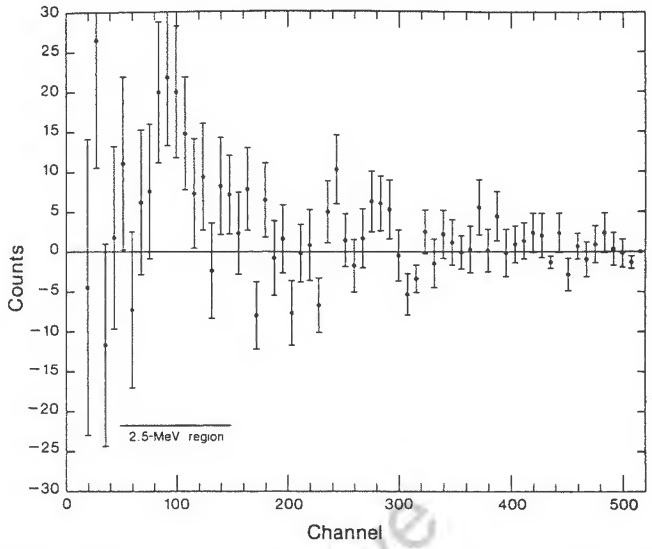

FIG. 3 Difference spectrum obtained by substracting scaled background from the foreground. Statistical errors ($\pm 1\sigma$) are shown for each eight-channel bin.

obtained for each run. Electronic changes were made in the apparatus during the course of the experiment which altered the observed background rates, so we plot the data in terms of foreground-to-background ratios rather than absolute rates. In one set of data (runs 1 to 8) for which the system was kept as untouched as possible to avoid changes in background rates, the measured rate of detection of 2.5-MeV neutrons was $(6.2 \pm 1.3) \times 10^{-4}$ s^{-1} above background. For this set of data, the background and foreground rates for all energies above ~3 MeV (that is, for all channels from 190 to 512) are equal, at $(1.4 \pm 0.1) \times 10^{-3}$ s^{-1}.

Run 6 is particularly noteworthy, with a statistical significance of approximately five standard deviations above background. Fused titanium pellets were used as the negative electrode, with a total mass of ~3 g. The neutron production rate increased after about one hour of electrolysis. After about eight hours, the rate dropped dramatically, as shown in the follow-on run 7. At this time, the surfaces of the titanium electrodes showed a dark grey coating. An analysis using electron microscopy with a microprobe showed that the surface coating was mostly iron, deposited with deuterons at the cathode. The same phenomenon of a decrease in the neutron signal after about eight hours of operation appears in run 13 followed by run 14. Runs 13 and 14 use the same eight electrochemical cells, and again the negative electrodes developed coatings after a few hours of electrolysis. These observations suggest the importance of surface conditions for the cold fusion process. Variations in surface conditions and electrolyte composition are anticipated during each test run because materials plate out of solution; the solution pH also changes significantly during a run. These 14 runs represent two choices of electrode material plus various operating currents. These variations may account for the fluctuations in the signal level that are evident in Fig. 4. As these runs represent a total of only ~200 signal neutrons at an average rate of ~2 per hour, it was difficult to optimize experimental conditions. This is a task for future research.

The observed 'turning off' of the signal after about eight hours may account for low signal-to-background ratios in runs 1 and 3, in that a signal that lasted for only a few hours may have been overwhelmed after a long (~20-hour) running time. When run 10 started with rates substantially above background, we stopped the run and removed half of the electrochemical cells as a test. The neutron production rate dropped off as expected (run 11). In determining the statistical significance of the data, we included runs 1, 3, 7, 11 and 14, even though we see a

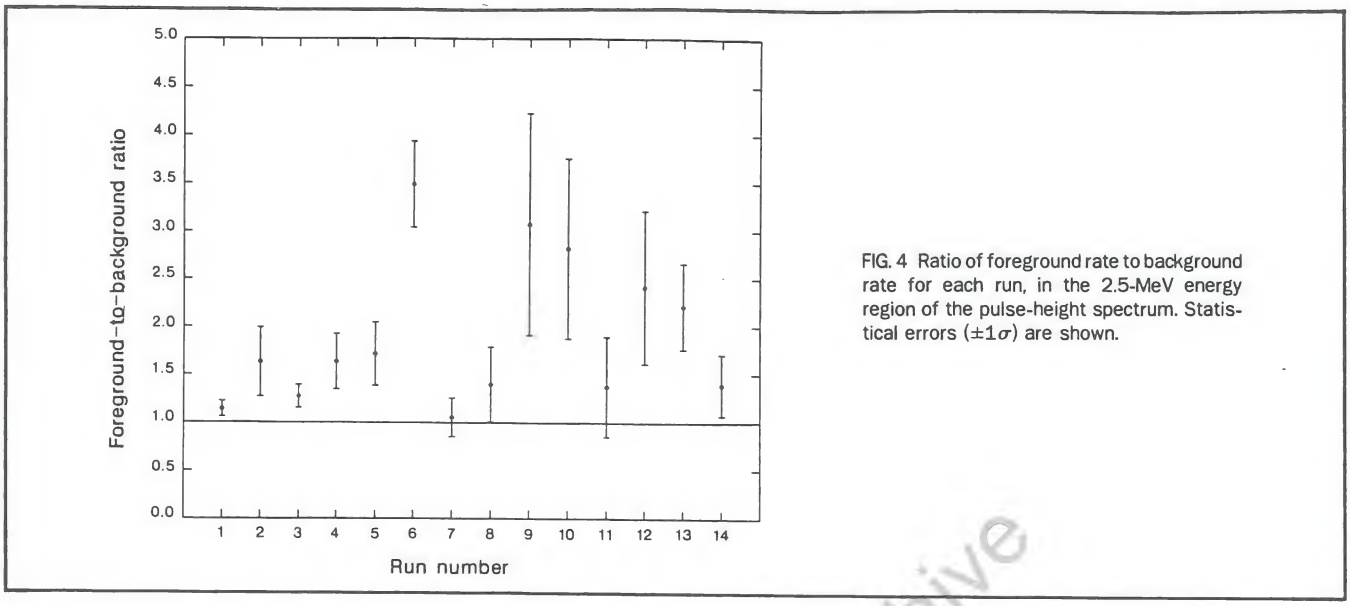

FIG. 4 Ratio of foreground rate to background rate for each run, in the 2.5-MeV energy region of the pulse-height spectrum. Statistical errors ($\pm 1\sigma$) are shown.

systematic reason for their low foreground-to-background ratios as explained above. Run 8, shown in Fig. 4, was inadvertently lost from the magnetic storage device and could not be included in Figs 2 and 3. This does not change our conclusions.

We can estimate the rate for the neutron-production branch of d-d fusion during electrolysis, specifically for run 6, as follows:

$$\text{Fusions per deuteron pair per second} = \frac{R/\varepsilon}{M \times \frac{d}{2M}} \quad (4)$$

where the observed rate of neutron detection, $R = (4.1 \pm 0.8) \times 10^{-3}\ s^{-1}$, is based on foreground minus corresponding background counts in channels 45–150; the neutron detection efficiency, including geometrical acceptance, is calculated using a Monte Carlo neutron-photon transport code[11] to be $\varepsilon = (1.0 \pm 0.3)\%$; $M \approx 4 \times 10^{22}$ titanium atoms for 3 g of titanium; and the ratio of deuteron pairs to metal ions, $d/2M \approx 1$, is based on the assumption that nearly all tetrahedral sites in the titanium lattice are occupied, forming the γ-TiD_2 hydride. Then the estimated cold nuclear fusion rate for the neutron-production branch, by equation (4), is $\lambda_f \approx 10^{-23}$ fusions per deuteron pair per second. If most fusions take place near the surface, or if the titanium lattice is far from saturated with deuterons, or if conditions favouring fusion occur intermittently, then the inferred fusion rate must be much larger, perhaps 10^{-20} fusions per deuteron pair per second.

We note that such a fusion rate could be achieved by 'squeezing' the deuterons to about half their normal (0.74-Å) separation in molecules. That such rates are now observed in condensed matter suggests catalysed 'piezonuclear' fusion as the explanation[1]. A possible cause is that quasi-electrons form in the deuterated metal lattice, with an effective mass a few times that of a free electron. Isotopes of hydrogen are known to accumulate at imperfections in metal lattices[12], and a local high concentration of hydrogen ions might be conducive to piezonuclear fusion. Because we have not seen any evidence for fusion in equilibrated, deuterated metals or compounds such as methylamine-d_2 deuteriochloride or ammonium-d_4 chloride, we conclude that non-equilibrium conditions are essential. Electrolysis is one way to produce conditions that are far from equilibrium.

It may seem remarkable that one might influence the effective rate of fusion by varying external parameters such as pressure, temperature and electromagnetic fields, but just such effects are seen in another form of cold nuclear fusion, muon-catalysed fusion[13].

Conclusions

The correlation of ideas regarding cold piezonuclear fusion[1] with observations of excess ^3He in metals and in geothermal areas of the Earth led to our experimental studies of fusion in electrochemical cells, which began in May 1986. Our electrolyte compositions evolved from geochemical considerations, and changed as results were observed. The presence of a fusion neutron signal was consistently reproduced, although the rate varied widely. Now that our exploratory searches have disclosed a small piezonuclear fusion effect, it remains to disentangle the factors that influence the fusion rate.

The need for off-equilibrium conditions is clearly implied by our data, and suggests that techniques other than electrochemistry may also be successful. We have begun to explore the use of ion implantation and of elevated pressures and temperatures, mimicking geological conditions. Cold nuclear fusion in condensed matter may be of interest as a novel probe of metal–hydrogen systems, including geological ones, and as a source of monoenergetic neutrons. If deuteron–deuteron fusion can be catalysed, then the d-t fusion reaction is possibly favoured because of its much larger nuclear cross-section. Although the fusion rates observed so far are small, the discovery of cold nuclear fusion in condensed matter opens the possibility, at least, of a new path to fusion energy. □

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ACKNOWLEDGEMENTS. We acknowledge valuable contributions of James Baer, David Mince, Rodney Price, Lawrence Rees, Eugene Sheely and J. C. Wang of Brigham Young University, and of Mike Danos, Fraser Goff, Berndt Müller, Albert Nier, Göte Ostlund and Clinton Van Siclen. We especially thank Alan Anderson for advice on the data analysis and Harmon Craig for continuing encouragement. This research is supported by the Advanced Energy Projects Division of the US Department of Energy.

■ **For a comment from one of the referees of this paper, please see page 711.**

Italian Researchers Report Achieving Nuclear Fusion

Reuters

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simple
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By MARLISE SIMONS
Special to The New York Times

ROME, April 18 — Italian Government scientists said today that they had achieved nuclear fusion in a simple experiment related to one carried out by scientists in Utah, but that they had used different materials and techniques.

The scientists said they had obtained the largest quantity of neutrons, an important indicator of nuclear fusion, that had yet been measured in this type of experiment. They said the level of neutrons was more than 100 times that of background radiation.

They described their findings as a purely scientific advance, cautioning that it was too early to tell what practical application the work might have. But they said it might open the way for new experiments in the search for cheap nuclear energy.

Effort by 9 Scientists

Dr. Francesco Scaramuzzi, a mathematician and physicist at the National Agency for Nuclear and Alternative Energy, said he and a team of eight other scientists had forced deuterium, a heavy isotope of hydrogen, in the form of gas to fuse at extremely low to room temperatures.

Fusion is the process that powers the sun, stars and hydrogen bombs, fusing atoms together rather than breaking them apart as is done in nuclear reactors. The achievement of fusion usually takes hundreds of millions of degrees of heat, and its controlled release on earth has so far eluded scientists, despite decades of effort.

On March 23 two researchers, Dr. B. Stanley Pons of the University of Utah and Dr. Martin Fleischmann of the University of Southampton in England, announced that they had created fusion energy in a jar of heavy water at room temperature.

Several laboratories around the world have reported repeating the experiments and obtaining similar results, but many physicists who are experts in fusion have said that they remain skeptical.

A previous statement by researchers at the Georgia Institute of Technology that they had detected neutrons from a cold fusion reaction was subsequently withdrawn after they learned that their detector was faulty.

The Italian scientists made their announcement in a news conference at the Rome headquarters of the Government nuclear energy agency. They were accompanied by a large number of colleagues and two cabinet ministers.

Plan to Publish Findings

Dr. Umberto Colombo, president of the agency, said that before the announcement today, a patent was sought for the technique. Full details of the experiment he said, will be published soon in the journal Europhysics Letters.

Explaining his experiment, Dr. Scaramuzzi said that he had exposed titanium scraps to deuterium gas. He said the titanium scraps were used to increase the surface area that would come into contact with the gas. Unlike the scientists in Utah, the Italian researchers said, they did not conduct an electrochemical experiment and used no electrolytic cell and no electricity..

Instead, the Italian scientists said that they had changed temperatures and pressures to achieve fusion. In one experiment, the pressure was kept high while the temperature drifted up and down, with the lowest temperature being 200 degrees centigrade below zero, the temperature of liquid nitrogen.

In a second experiment, the liquid nitrogen was removed so that the temperature rose toward room temperature, and the deuterium gas was also removed.

Dr. Colombo said the team was unable to define the precise point of temperature or pressure that caused the fusion. "We believe it was the dynamic condition that was more important than the precise point of the pressure or of the temperature, he said.

Theory on What Happened

Dr. Scaramuzzi said: "Our interpretation is that in the first experiment there was absorption of deuterium. In the second test the metal was getting rid of the deuterium." The second test produced the most neutrons.

> A high level of neutrons is cited to show that it was not chemical.

The enthusiasm of the Italian team was largely prompted by the high level of neutrons.

Dr. Roberto Andreani, director of the department of fusion at the agency's research facility at Frascati, said that in the first experiment 20 to 40 neutrons were measured in successive 10-minute periods. This compares with a background level of 2 neutrons that are naturally present from cosmic rays.

In the second experiment, researchers measured flows of 200 to 300 neutrons every 10 minutes for more than 12 hours, which was more than 100 times the background level, he said.

Despite the high neutron yield, the team members said they had obtained virtually no energy.

Stanford Reports Success

By WILLIAM J. BROAD

A team of scientists at Stanford University said yesterday that they had duplicated the experiment in which nuclear fusion was reportedly achieved in a jar of water at room temperature. The Stanford researchers said they measured heat but not radiation or subatomic particles that are often produced by nuclear fusion.

"This is an important confirmation" of cold fusion, the team's leader, Robert A. Huggins, said in an interview. Dr. Higgins, a professor of materials science and engineering at Stanford, added that his team's work involved a controlled experiment that ruled out the possibility that the reaction under study was chemical rather than nuclear. But other scientists disagreed with Dr. Higgins's analysis.

The Stanford work aimed at duplicating the efforts of Dr. Pons and Dr. Fleischmann. Dr. Huggins and his six-member team began their experiment on April 5, and it eventually including five separate electrolytic cells. As a way to check to see if the sought-after reaction was chemical or nuclear, the experiment included not only flasks filled with heavy water but also ones with regular water that contained no added deuterium. As evidence of the fusion reaction, the Stanford scientists looked for heat production.

The results, Dr. Huggins said, were an excessive heat production in the flasks containing heavy water, but nothing like that in the ones filled with regular water.

"As far as we can tell, we've got a direct comparison," Dr. Huggins said. "This negates any possible chemical effects," adding that "this is the comparison that the physics community insisted upon."

He said the amount of heat energy produced was "in the general range" of that reported by Dr. Pons and Dr. Fleischmann.

George Chapline, a physicist at the Lawrence Livermore National Laboratory in California, said that the Stanford experiment "doesn't necessarily prove" that nuclear reactions are the cause of the heat.

Price Riot in Jordan

AMMAN, Jordan, April 18 (Reuters) — Riots over price increases erupted in Maan in southern Jordan today and at least 17 people were injured as some 4,000 people attacked shops and set cars on fire, a hospital source said. Security forces opened fire to disperse the demonstrators, the source said. Reached by telephone from Amman, the source said 17 people were being treated at a hospital in Maan.

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them, an Administration official said.

American officials familiar with the study say that the longer range of the successor to the Lance would make it possibile to deploy the weapon behind the front lines, making it potentially less vulnerable to attack. This would enable NATO to eliminate some old artillery shells.

The NATO Secretary General, Manfred Wörner, said in an interview last week that one element of a compromise at the NATO summit meeting next month could involve a "general declaration that as a consequence of modernization there can be further unilateral reductions, especially in the field of artillery."

Mr. Wörner said other elements of a compromise could involve a "general assurance" by NATO that its remaining nuclear weapons would be kept "up to date." No specific reference to a successor for the Lance would be made.

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Garwin RL 04/23/89 2 042389.RLG oh Foils used by RLG in Cold Fusion session at NAS 04/24/89.
Hagelstein PL 04/10/89 18 041089.PLH cp "A Simple Model for Coherent DD Fusion in the Presence of a Lattice," MIT.
Hagelstein PL 04/00/89 11 040089.PLH cp "Phonon Interactions in Coherent Fusion," MIT.
Hudson RL 04/20/89 1 042089.RLH cn "Pons and Fleischmann Withdraw Fusion Paper from UK Journal" in WALL STREET JOURNAL.
Qiu ZM 04/19/89 2 041989.ZMQ il Thanks for your 04/16 FAX. Re cold nuclear fusion experiments in China.
Salvetti C 04/24/89 1 042489..CS cw (W/attach 040089FRAS) To R. DiMenza. Re cold nuclear fusion.
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Received: from mike.UCSC.EDU by ucscc.UCSC.EDU (5.61/1.35)
id AA14134; Mon, 24 Apr 89 11:04:52 -0700
Received: by mike.UCSC.EDU (3.2/3.14)
id AA14814; Fri, 24 Mar 89 09:26:36 PST
Date: Fri, 24 Mar 89 09:26:36 PST
From: michael@chromo (Michael Nauenberg)
Daemon: Maxwell's
Return-Path: <michael@chromo>
Message-Id: <8903241726.AA14814@mike.UCSC.EDU>
To: rlg2@yktvmv.bitnet
Subject: Cold Fusion Nonsense

Dear Richard,

Steve Koonin has been forwarding to me your
e-mail messages which I find very interesting.
Regarding your last one about Paneth and Peters
paper on hydrogen conversion to helium you probably
know by now that the Swedish scientist John Tandber
made similar experiments with Palladium. Tandber
applied for a patent which was rejected.
I am convinced that Pons and Fleisham are
flakes, and I can't understand how they continue
to get such attention. Enclosed are some of my comments
about Jones talk at UCSB which migth interest you.

I would appreciate it if you could add my name
to your e-mailing list:

nauenbe@ucscd.bitnet

Regards,

Michael (Nauenberg)
Hi Hal,

I agree with your view of Jones's talk. He has convinced
himself that a marginal experiment is right, although
no one has been
able to verify it. He was more interested in talking
to newsmen than to the ITP people who invited him.
From his talk one did not learn anything new.
However, since their announcement
they have found out (after questions by outside experts) that
3/4 of their background were misidentified as gammas instead
of neutrons. They have quietly put out a second preprint
which doesn't mention their previous error.
Even if this does not affect their conclusions, it shows
carelesness in such an important matter.
Their claim of 5 standard deviations
depends on massaging their background. It looks more like
3 sigmas. Particle physicists
know that better peaks than theirs,
which are routinely found in particle physics, disappear
quietly without any fuzz. I was particular struck that
the motivation for his experiment came from their calculation
in 1986 which we have shown to be wrong by ten orders of
magnitude. In particular, his claim that scaling by a
factor of two leads to his so called observed rate is
wrong. The scaling factor is 5, not 2.

I checked that this mistake was first made in the 1960 paper of Zeldovich and Gershtein (who incidentally coined the term piezonuclear which Jones credits to himself). They must have simply copied the Russian result without checking it.
Garwin reports that a Russian physicist at Erice reached the same conclusion as Koonin and I. So why does Jones, who attended the Erice meeting, continues to talk about the wrong factor?
In short, Jones appears to me to have little respect for theoretical and possibly experimental numbers which don't fit his views.

I almost wasted $200 in flying out for the day, except that I had the pleasure to meet and talk with C. Barns from Cal Tech who is an expert nuclear physicist.

Regards,

Michael

Received: from CUNYVM by CUNYVM.BITNET (Mailer R2.01) with BSMTP id 8715; Mon, 24 Apr 89 09:21:41 EDT
Received: from rocky2 by CUNYVM.CUNY.EDU (IBM VM SMTP R1.1) with TCP; Mon, 24 Apr 89 09:20:29 EDT
Received: by rocky2 (5.52/5.17)
    id AA21905; Mon, 24 Apr 89 09:01:30 EDT
Received: by casp1.rockefeller.edu (3.2/SMI-3.2)
    id AA12061; Mon, 24 Apr 89 09:00:25 EDT
Message-Id: <8904241300.AA12061@casp1.rockefeller.edu>
To: rlg2@yktvmv.BITNET
Reply-To: lederberg@ROCKY2.ROCKEFELLER.EDU
Cc: system@ROCKY2.ROCKEFELLER.EDU
Subject: Re: Address problems.
In-Reply-To: Your message of 24 April 1989, 00:14:53 EDT.
            <8904240417.AA17824@rocky2>
Date: Mon, 24 Apr 89 09:00:22 -0500
From: jsl@casp1.rockefeller.edu

Dick -- I think my domain doesn't recognize extended addresses like:
cc: joshua.s.lederberg.570-8080.jsl@rocky2.rockefeller.edu

Leave it as I indicated on "Reply-to" above. Where did you get the long string -- so I can correct others' listings?

It would take:
cc: (joshua.s.lederberg.570-8080)jsl@rocky2.rockefeller.edu
  i.e. ignoring the ()

The Chronicle of Higher Education also referred to a Swedish patent on fusion in palladium -- perhaps the same work you cited.

Pons and Fleischmann Withdraw Fusion Paper From U.K. Journal

By Richard L. Hudson
Staff Reporter of The Wall Street Journal

LONDON—The two scientists who discovered a controversial "cold fusion" nuclear reaction withdrew their paper from publication in a major scientific journal after declining to answer its questions.

The surprise withdrawal from Britain's Nature magazine further fuels a three-week-old controversy over the reliability of the scientists' results.

The two chemists, B. Stanley Pons of the University of Utah and Martin Fleischmann of the University of Southampton in England, claim to have found a new type of nuclear reaction at room temperature that produces more energy than it consumes—a decades-old goal of nuclear physicists that could herald a new and inexpensive source of energy.

Publication Had Been Expected

Nature, a respected 130-year-old journal, had been expected to publish a scientific account by Messrs. Fleischmann and Pons of their experiments. But Nature's editor, John Maddox, said three of the journal's scientist-reviewers raised questions about the research that the two authors declined to address. Critical peer reviews are standard procedure for scientific journals, and often end in rejection or withdrawal. But it's rare for a paper of such importance to be pulled back from the presses.

"Fleischmann and Pons have taken the view that they couldn't at the same time satisfy the [Nature] referees and get on with other urgent work," the journal said in an editorial to be published today.

The journal said the decision shouldn't be interpreted as casting doubt on the researchers' credibility. It is the prerogative "of authors to decide whether it is worthwhile to reply to referees' comments," the journal said. But the development will doubtless intensify skepticism about their work. Despite attempts by more than 60 labs world-wide to reproduce their results, only one—at Stanford University—has publicly claimed to have seen anything resembling the energy-output Messrs. Fleischmann and Pons said they detected.

Resentment Among Scientists

Nature's reservations about the paper come at a time of growing resentment among scientists over the fact that Messrs. Pons and Fleischmann haven't yet disclosed crucial details of their experiment, possibly causing hundreds of researchers trying to replicate it to waste time doing studies the wrong way.

"It's tremendously irritating that Pons and Fleischmann left out details" in their initial report, said Kevin Myles, a researcher at Argonne National Laboratory near Chicago. He added that Messrs. Pons and Fleischmann already have established themselves as originators of practical cold fusion—if the phenomenon exists—and so should have no patent-related reasons for withholding details of their work. The two "owe it to the scientific community to have open disclosure on what is wrong" with the dozens of experiments that have failed to show cold fusion, said Kelvin Lynn, a researcher at Brookhaven National Laboratory in Upton, N.Y.

Mr. Fleischmann didn't return phone calls seeking comment on the article and Mr. Pons couldn't be reached.

Other Fusion Research

Another set of cold-fusion experiments has gained wider scientific support, and Nature said it will publish a paper on that research next week. The paper, by Steven Jones and colleagues at Brigham Young University in Provo, Utah, reports fusion at room temperature, but without the huge energy output claimed by Messrs. Fleischmann and Pons. The Jones group, Nature said, "have been able to amend their text in a way that satisfies the referees."

The points raised by Nature's reviewers appear to have been the same ones raised by scientists world-wide. Nature's Mr. Maddox said they included "criticisms about the lack of control experiments." The reviewers sought, for instance, any results of the cold-fusion experiments using ordinary water instead of "heavy" water. In the experiments, water with heavier-than-normal hydrogen atoms is drawn to a palladium electrode where, the scientists claim, they fuse. A test with plain water could check for some overlooked chemical reaction that might explain the results.

Another reviewer criticism, Mr. Maddox said, was inadequate data on whether some of the experiments' mysterious heat energy might be coming simply from electricity passing through the heavy water, rather than from an exotic nuclear reaction. Mr. Maddox added that the article as submitted wouldn't have told scientists more than they already knew. He said the paper was a shortened version of a report published in the Journal of Electroanalytical Chemistry, a Swiss publication.

Underlining Nature's skepticism is a critical review it is also publishing of the Fleischmann-Pons work. The review, by a prominent U.S. physicist, Richard L. Garwin, analyzes several possible flaws with the scientists' work. In it, Mr. Garwin, former director of International Business Machines Corp. main science lab, concludes the discovery could be "a multidimensional revolution." But, he added, "I bet against its confirmation."

Foxboro, Chino Plan Venture

FOXBORO, Mass.—**Foxboro Co.** and **Chino Corp.**, of Tokyo, announced a joint-venture agreement under which they will make in Japan Foxboro's Intelligent Automation Series Systems for sale in the Japanese market.

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Corporate Scientists Seeking to Confirm 'Cold Fusion' Report No Successes So Far

By Amal Kumar Naj
Staff Reporter of The Wall Street Journal

Scientists at a number of major corporations are trying to check the validity of the controversial University of Utah "cold fusion" experiment, so far without reported success.

International Business Machines Corp. yesterday went so far as to issue pre-prints of a paper that its scientists are submitting to a scientific journal on their failure to duplicate the experiment by Utah chemist B. Stanley Pons and British chemist Martin Fleischmann. The two chemists announced three weeks ago their battery-like laboratory device was producing four times as much energy as it was consuming through some previously undiscovered nuclear reactions involving the fusing of "heavy" hydrogen (deuterium) atoms.

The IBM researchers say in their paper that despite trying several possible variations of what is known about the Utah experiment, and despite using instruments more sensitive than those used in Utah, "our results show that no excess heat and no nuclear fusion products were detected in any of the samples tested."

An IBM spokesman said that despite the researchers' failure to find signs of cold fusion, they plan to try other experiments that might produce the phenomenon.

At **Westinghouse Electric** Corp. in Pittsburgh, "we're working like mad trying to confirm the results," said Armand Panson, a researcher. Mr. Panson and his associates began their experiments on a Saturday, the day after the Utah claims were made in the press on March 24. Westinghouse has assigned 20 people to the project. "There is no question we are going after it aggressively," Mr. Panson said.

Westinghouse, of course, is a major builder of atomic power plants based on fission, or the splitting of atoms, as opposed to the fusion of atoms claimed by the Utah scientists. "If this is an alternative source of power, we want to be out there first," Mr. Panson said.

Similarly, **General Electric** Co. also is pursuing the Utah experiments, but a spokesman declined to discuss the GE effort, saying that results weren't ready to be reported.

Other companies that are carrying out cold fusion experiments or that are preparing to conduct tests include **Engelhard** Corp., a specialty metals producer; **KMS Industries** Inc., which created the first laser-initiated thermonuclear reaction; and **American Telephone & Telegraph** Co.'s Bell Laboratories unit.

Engelhard has a direct interest in the Utah experiment because key components are made of palladium and platinum, metals that Engelhard refines. At least one chemical company is considering a fusion experiment because it produces "rare earths" that might be a substitute for palladium in any cold fusion device.

As in the case of university scientists, the corporate researchers say their efforts are being hamstrung by the failure of Messrs. Pons and Fleischmann to publish details on exactly how they set up their experiments. Some say they were confounded by the lack of clear answers to the barrage of questions addressed to Mr. Pons at the American Chemical Society's annual convention in Dallas last week.

For example, notes one industrial chemist, Mr. Pons didn't identify the precise isotopic nature of the lithium electrolyte used in his experiment and couldn't explain why the palladium in the device hardened significantly after the experiment had run for several weeks.

"It was unbelievable to me that over and over again he said he hadn't yet done this or that, or he was looking into it. If they were working for four years on this, why didn't they know these simple things?" asked Mr. Panson.

Harmon Garfinkel, vice president of research and development at Engelhard, said, "I don't know what drove Pons-Fleischmann to rush" their results into print without describing their methods.

Nevertheless, he added, "it doesn't serve any purpose to say it's all nonsense. So we are going ahead and doing the appropriate experiment ourselves."

—David Stipp contributed to this article.

Philips to Reorganize World-Wide Business In Integrated Circuits

By a Wall Street Journal *Staff Reporter*

BRUSSELS—In line with its drive to streamline global operations, N.V. Philips said it would reorganize its world-wide integrated-circuit business by the end of 1990.

Electronics-industry analysts welcomed the move, saying it was a step in strengthening Philips's international position and possibly in improving a weak sales and marketing organization in the integrated-circuit business.

Philips is Europe's leading semiconductor manufacturer, with $1 billion in European sales last year, according to a U.S. market research concern, Dataquest Inc. The Dutch electronics giant ranks No. 10 world-wide, with $1.76 billion in 1988 semiconductor sales. That 1988 global-sales figure was a rise of only 10%, while the world market grew 31.9% to $50.49 billion, according to Dataquest.

In the future, two newly created business units will have world-wide responsibility for designated products, according to the plan.

OPTIONS: NOACK LOG LONG NOTEBOOK *
Local options: Search RealNode

Date: 20 April 1989, 12:06:48 EDT
From: R.L.Garwin (914) 945-2555 RLG2 at YKTVMV
P.O. Box 218
Yorktown Hts, NY 10598
To: RSIMON at NAS
cc: Jacob Bigeleisen (516) 632-7905 JBIGELEI at SBCCMAIL
Subject: Walling talk.

Jacob Bigeleisen has invited me to the Sunday Walling talk at 12:00.
I have been in touch with Walling, and I shall attend.

Thanks very much.

Dick Garwin

Date: Thu, 20 Apr 89 10:47 EDT
From: <JBIGELEI@SBCCMAIL>
Subject: Scheduled Report by Walling at NAS Meeting
To: rlg2@yktvmv
X-Original-To: rlg2@yktvmv.bitnet

State University of New York at Stony Brook
Stony Brook, NY 11794-3400

Jacob Bigeleisen
Professor
Chemistry
516-632-7905
20-Apr-1989 10:42am EDT

FROM: JBIGELEISEN

TO: Remote Addressee (_RLG2@YKTVMV.BITNET)
CC: Remote Addressee (_WSPINDEL@NAS.BITNET)

SUBJECT: Scheduled Report by Walling at NAS Meeting

Dear Dick:
I have received the following information about a presentation to be made by Cheves Walling to the Board of Chemistry and Chemical Technology of CPSMR, formerly AMPS. The meeting will be held at the One Washington Cirle Hotel. There is a buffet lunch at 12 noon to which I am inviting you. If you plan to attend, please notify RSIMON@NAS.BITNET JAKE

Received: from JNET-DAEMON by ccmail.sunysb.edu; Tue, 18 Apr 89 15:18 EDT
Received: From NASVM(MAILER) by SBCCMAIL with Jnet id 4673 for JBIGELEISEN@SBCCMAIL; Tue, 18 Apr 89 15:18 EDT
Received: by NASVM (Mailer X1.25) id 4672; Tue, 18 Apr 89 15:15:21 EDT
Received: by NAS (PC Mail Gateway) id 6019; Tue, 18 Apr 89 15:15:15 EDT
Date: Tue, 18 Apr 89 14:04 EDT
From: William Spindel <WSPINDEL@NAS.BITNET>
Subject: About the cold fusion---hear from a source
To: JBIGELEI@ccmail.sunysb.edu

*cc: Robert M. Simon
*cc: Norman Metzger at NRC/EXOF
*cc: Allan Hoffman

Your NAS Colleague and former JACS editor, Cheves Walling, is a collaborator on the U of Utah experiments and will report to NAS chemists at the BCST meeting at about 1 pm Sunday. Tell him your views.

Date: Thu, 20 Apr 89 07:07:36 PDT
From: hlewis@sbphy.ucsb.edu (hal lewis)
Subject: RE: Contact.
To: rlg2%ibm.com%lbl.gov%sbphy.ucsb.edu@voodoo.ucsb.edu

Yes, it reached me, and thanks.
Either hlewis@sbphy.ucsb.edu or hlewis@sbitp.bitnet will work.
What I used for you was rlg2@ibm.com, which apparently worked.

The non-solid-state types in the cold fusion game don't seem to understand that large effective masses depend on coherent interactions with the lattice, and can't work in the small.

Even in the light of the morning after, Jones was awful yesterday.
And it was a media event, but I don't blame him for that.

I'll be in Wash tomorrow, back Friday night.
Cheers,
Hal

Date: Thu, 20 Apr 89 10:47 EDT
From: <JBIGELEI@SBCCMAIL>
Subject: Scheduled Report by Walling at NAS Meeting
To: rlg2@yktvmv
X-Original-To: rlg2@yktvmv.bitnet

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Stony Brook, NY 11794-3400

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From: William Spindel <WSPINDEL@NAS.BITNET>
Subject: About the cold fusion---hear from a source
To: JBIGELEI@ccmail.sunysb.edu

*cc: Robert M. Simon
*cc: Norman Metzger at NRC/EXOF
*cc: Allan Hoffman

Your NAS Colleague and former JACS editor, Cheves Walling, is a collaborator on the U of Utah experiments and will report to NAS chemists at the BCST meeting at about 1 pm Sunday. Tell him your views.

Enhancement of Cold Fusion Rates by Fluctuations

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Santa Barbara, CA 93106

(Submitted to *Physical Review Letters*, April 19, 1989)

ABSTRACT

The rate at which two nuclei tunnel through the coulomb barrier inhibiting their fusion can be enhanced significantly by modest fluctuations in their environment. Such enhancements might play a role in recent experiments claiming to observe cold fusion.

Recent reports have suggested the observation of fusion of deuterium nuclei introduced by electrolysis into solid Palladium [1] and Titanium [2]. The fusion rates implied by these results are 40–50 orders of magnitude larger than might be expected naively [3,4], although the temperatures involved are far too low to achieve such rates by the conventional thermonuclear mechanism of surmounting the coulomb barrier [5]. Thus, if these experiments are indeed revealing nuclear processes, alternative explanations must be sought. In this Letter, I suggest that modest fluctuations in the environment of the fusing nuclei can lead to the required enhancements. The mechanism is closely related to an explanation of large sub-barrier enhancements of heavy-ion fusion cross sections [6].

The rate at which two nuclei will fuse depends upon the extent to which they tunnel through the potential $V(\mathbf{r})$ describing their interaction as a function of their separation $\mathbf{r}$. In an isolated diatomic molecule, the electronic time scale τ_e is far shorter than the fusion time scale τ_f (i.e., the "time" the system is in a classically forbidden region), and $V(\mathbf{r})$ can be taken to be the usual Born-Oppenheimer potential. Indeed, a simple estimate is $\tau_f/\tau_e \approx \mu^{1/2}$, where μ is the reduced mass of the fusing nuclei. [Note that I am using atomic units where $\hbar = m_e = e = 1$, so that $\mu = M_n A_1 A_2/(A_1 + A_2)$, where $A_{1,2}$ are the mass numbers of the fusing nuclei and $M_n = 1833$ is the ratio of the nucleon and electron masses. Lengths are therefore measured in Bohr radii (0.53×10^{-8} cm),

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energies are measured in Hartrees (27.2 eV), and times are measured in $\tau_e = \hbar/(27.2\text{eV}) \approx 2.5 \times 10^{-17}$ s.]

For two hydrogen nuclei in a solid, I must consider the effect of all other degrees of freedom beyond $\mathbf{r}$. The electronic coordinates are still safely adiabatic, but the coordinates of the other nuclear degrees of freedom are likely not. For a solid composed of nuclei of mass number A_L, the lattice (phonon) time scale is $\tau_L \approx A_L^{1/2} M_n$, the time scale for the center-of-mass motion of the fusing pair is $\tau_{CM} \approx (A_1 + A_2)^{1/2} M_n$, and the time scale for motionof the other hydrogen nuclei is $\tau_H \approx A_{1,2}^{1/2} M_n$. Typically, I expect $\tau_L \gg \tau_f$ ($A_L = 106$ for Pd), and $(\tau_{CM}, \tau_H) \gtrsim \tau_f$.

Although methods for treating tunneling in general multi-dimensional situations have been developed using functional-integral methods [7], the sudden limit is one plausible and tractable approach to the present problem. The tunneling of the fusing nuclei must be calculated in an instantaneous potential $V(\mathbf{r}; \xi)$ that depends parametrically upon the non-adiabatic coordinates, which we have denoted collectively by ξ. Thus, $V(\mathbf{r}; \xi)$ as a function of ξ embodies variations of the interaction potential between the fusing pair associated with their location within the lattice, the shape of the interstitial cavity in which they might be contained, the possible presence of a third hydrogen nucleus nearby, etc. For $r \ll 1$, I expect the inter-nuclear coulomb repulsion to dominate, so that

$$V(\mathbf{r}; \xi) \approx \frac{1}{r} + V_0(\xi) \, , \tag{1}$$

while for distances smaller than the electronic screening length, $V(\mathbf{r}; \xi)$ can be expanded in terms of the instantaneous local electric field, quadrupole field, etc.

The coordinates ξ are not fixed, but rather fluctuate on time scales longer than τ_f due to zero-point and thermal motion, as well as non-equilibrium conditions. If the instantaneous fusion rate of the pair is $\Lambda(\xi)$, and if $P(\xi)$ denotes the normalized probability distribution of the non-adiabatic coordinates, then the effective fusion rate is

$$\Lambda_{\text{eff}} = \int d\xi P(\xi) \Lambda(\xi) \, . \tag{2}$$

This expression is analogous to the usual one for thermonuclear rates in which the fusion cross section is averaged over a Maxwellian distribution

of relative velocities [6]. Here, however, the average is over the potential through which the nuclei tunnel, rather than their asymptotic kinetic energy.

The coordinates ξ can be defined such that $\xi = 0$ is the most probable configuration (e.g., as might be assumed for an interstitial pair in a fixed lattice), so that I can take

$$P(\xi) = \frac{1}{(2\pi\sigma^2)^{1/2}} e^{-\xi^2/2\sigma^2} , \tag{3}$$

with σ characterizing the scale of the fluctuations. Further, if we write

$$\Lambda(\xi) = Ae^{-2S(\xi)} \tag{4}$$

with A the nuclear rate constant and $e^{-2S(\xi)} = |\Psi(r = 0; \xi)|^2$ the probability to find the fusing pair at $r = 0$, then the change in the fusion rate due to fluctuations is

$$E = \frac{\Lambda_{\text{eff}}}{\Lambda(\xi = 0)} = \int \frac{d\xi}{(2\pi\sigma^2)^{1/2}} e^{-\xi^2/2\sigma^2 - 2[S(\xi) - S(0)]} . \tag{5}$$

It is easy to see that most typically $E > 1$, so that fluctuations enhance the effective fusion rate. For example, if a Taylor expansion of $S(\xi)$ about $\xi = 0$ is valid,

$$S(\xi) - S(0) \approx S'\xi + \frac{1}{2}S''\xi^2 + O(\xi^3) \tag{6}$$

where the derivatives are evaluated at $\xi = 0$, then

$$E \approx (1 + 2S''\sigma^2)^{-1/2} e^{2S'^2\sigma^2} . \tag{7}$$

Apart from the prefactor, which is unimportant for small σ, $E > 1$ and increases exponentially with σ^2. Moreover, as S is generally large, even small fluctuations in $V(\mathbf{r}; \xi)$ (i.e., small σ) can have a significant effect.

The magnitude of the fluctuations required to produce a given enhancement is then the crucial question. A full evaluation of E requires a realistic specification of $V(\mathbf{r}; \xi)$ and a numerical integration of the Schroedinger equation to find $S(\xi)$. However, for the present illustrative discussion, it is sufficient to adopt a shifted Coulomb potential characterized by a single non-adiabatic parameter

$$V(\mathbf{r}; \xi) = \left[\frac{1}{r} - U(1 + \xi)\right] \Theta(r^{-1} - U(1 + \xi)) , \tag{8}$$

(Θ is the unit step function) and the simple WKB approximation for the zero-energy penetration,

$$S(\xi) = (2\mu)^{1/2} \int_0^{[U(1+\xi)]^{-1}} dr \left[\frac{1}{r} - U(1+\xi)\right]^{1/2} = \pi \left[\frac{\mu}{2U(1+\xi)}\right]^{1/2} . \quad (9)$$

Here, the parameter U describes the average of the constant term $V_0(\xi)$ appearing in Eq. (1), and the fluctuations of this term are given by ξU. A rough estimate of U can be had by considering tunneling in the molecular Hydrogen potential [4], which results in $S = 2.07\mu^{1/2}$, so that $U = 1.15$, or about 31 eV. I adopt this value for the purposes of concreteness in the following discussion.

With the assumptions above, the enhancement is given by

$$E = \int \frac{d\xi}{(2\pi\sigma^2)^{1/2}} e^{-\xi^2/2\sigma^2 - \pi(2\mu/U)^{1/2}[(1+\xi)^{-1/2}-1]} . \quad (10)$$

A saddle-point evaluation of this integral proceeds by finding ξ^*, the most effective value of ξ, as a root of the equation

$$-\frac{\xi^*}{\sigma^2} + \frac{\pi(\mu/2U)^{1/2}}{(1+\xi^*)^{3/2}} = 0 \quad (11)$$

and then

$$E \approx [1 + \frac{3\pi}{4}(2\mu U)^{1/2}(1+\xi^*)^{-5/2}]^{-1/2} e^{-\xi^{*2}/2\sigma^2 - \pi(2\mu/U)^{1/2}[(1+\xi^*)^{-1/2}-1]} . \quad (12)$$

In Table I, I show the enhancement expected for the d + d and p + d fusion rates for various values of σ. Note that even relatively small fractional fluctuations ($\sigma \lesssim 0.5$, or a fluctuation in V_0 of 15 eV) can produce enhancements of some 30 orders of magnitude. The most effective values of ξ generally lie in the extreme wings of $P(\xi)$. Thus, even very improbable configurations can have a significant effect, as the tunneling rate is very sensitive to the potential. Also note that the precise relation between E and σ depends upon what I've assumed for $S(0)$ and the way in which fluctuations perturb the potential.

Are these fluctuations reasonable? A proton at a distance of 1 Bohr radius generates a potential of 1 in atomic units, so that given the possibility of multiple occupation of the interstitial sites or large distortions of the

lattice, $\sigma \sim 0.5$ is perhaps not too implausible. Further, it seems likely that the p + d system, with its non-vanishing electric dipole moment operator, will be influenced by fluctuations more than the d+d system, which couples only to the electric quadrupole field. Finally, I note that any conditions in the system that enhance fluctuations will also enhance fusion rates. These might include heating to increase the number of phonons, high hydrogen fractions to increase the multiple vacancy probability, and the flow of a current to induce gross motion of the hydrogen nuclei.

In conclusion, I have shown that fluctuations in the environment of a fusing pair of nuclei within a solid can significantly enhance the rate at which they fuse. This is because of the extreme sensitivity of the fusion rate to the effective potential barrier inhibiting fusion. In a schematic calculation for a shifted coulomb potential, I showed that a fractional *rms* fluctuation of the potential by only 0.1 (about 3 eV) will enhance the fusion rate by some 8 orders of magnitude and that fractional rms fluctuations of 0.5 will lead to enhancements of more than 30 orders of magnitude. While these results are suggestive, a more detailed calculation of the interaction between hydrogen nuclei in a solid (and their tunneling in the presence of non-adiabatic degrees of freedom) would be required to establish the relevance of this mechanism to cold fusion.

This work was support in part by National Science Foundation grant PHY82-17853 at Santa Barbara, supplemented by NASA funds, and by National Science Foundation grants PHY86-04197 and PHY88-17296 at Caltech.

References

* Permanent address: W. K. Kellogg Radiation Laboratory, Caltech 106-38, Pasadena, CA 91125

[1] M. Fleischmann, S. Pons, and M. Hawkins, J. Electroanal. Chem. **261** (1989) 301.

[2] S. E. Jones, E. P. Palmer, J. B. Czirr, D. L. Decker, G. L. Jensen, J. M. Thorne, S. F. Taylor, and J. Rafelski, University of Arizona preprint AZPH-TH/89-18, March, 1989 (submitted to *Nature*).

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[5] D. D. Clayton, *Principles of Stellar Evolution and Nucleosynthesis*, (McGraw-Hill, New York, 1968), Ch. 4.

[6] H. Esbensen, Nucl. Phys. **A352** (1981) 147.

[7] A. D. Caldeira and A. J. Leggett, Ann. Phys. (NY) **149** (1983) 374; R. P. Feynman and F. L. Vernon, Ann. Phys. (NY) **24** (1963) 118.

Table 1: Enhancement factors and most efective configurations for the truncated Coulomb potential, Eq. (8).

	$\sigma = 0.1$	0.2	0.3	0.4	0.5	0.6	0.7
p + d							
$\log_{10}$ E	6.8	13.9	18.9	22.6	25.4	27.7	29.6
ξ^*	0.44	1.05	1.62	2.17	2.69	3.20	3.69
d + d							
$\log_{10}$ E	8.6	17.1	23.0	27.2	30.5	33.1	35.2
ξ^*	0.49	1.13	1.75	2.33	2.89	3.24	3.94

.qq
&EXIT

THIS FILE HAS BEEN RECEIVED FROM BITNET

The file may be executable. Before removing this header you must understand what the code will do. You must also have the appropriate intellectual property agreements in place before receiving the code into IBM.

If you have any questions, contact your manager.

The contents of the file has been shifted right by one character.
Filename=(none) Filetype=(none) RECFM=F LRECL=80 Records=40
The file received from the BITNET gateway begins below the next line.

Received: by talcott.harvard.edu; Wed, 19 Apr 89 08:54:38 EDT
Received: by cfa; Wed, 19 Apr 89 08:55:38 edt
Date: Wed, 19 Apr 89 08:55:38 edt
From: wpress%cfata2.DECNET@harvunxw.BITNET (WPRESS@CFA wpress@cfa.harvard.edu 66
98::WPRESS)
To: cfairt::rlg2@yktvmv.bitnet
Subject: 2 things: fusion and Carl Young (which is more surreal?)

From: CFATA2::WPRESS "WPRESS@CFA wpress@cfa.harvard.edu 6698::WPRESS" 19
PR-1989 08:53
To: CFAIRT::"rlg2@yktvmv",WPRESS
Subj: 2 things: fusion and Carl Young (which is more surreal?)

1. Thanks for your message re Cheves Walling. I am also in receipt (second hand) of your summary to Nature. Could you add me to your bitnet mailing list on this, so that I get any future things first-hand?

   The Walling thing is disturbing, because it is the first evidence of heat that is not so obviously flawed as to be dismissable. However, shouldn't I be suspicious of ANY mass spectrometer that sees He4 at 10¬12 number concentration, just because He4 is commonly used to find vacuum leaks, etc., so at some level it has probably soaked into every metal surface in the lab equipment? I agree with you it is supremely hard to not get hard X-rays or gammas. Suppose there were some channel that didn't produce "any". Then surely there should be a channel down by only 137 or 137¬2 which DOES produce them -- as a QED correction -- and even this would be way too much, no?

2. Carl Young (whom Paul and I do some business with, you recall) wants you, Paul, and me to meet with some high-ups in his organization in D.C. some time in May. The purpose of the meeting is not substantive, i.e. it is to "sell" JASON and (equally importantly) the specific people who might be cleared into the relevant program. In other words, they need to see that you are not the ogre that some say you are. Do you have any dates that you will be in D.C. for another purpose?

Cheers, -B.

Received: from Erin.Caltech.Edu by Iago.Caltech.Edu with DECNET ;
Wed, 19 Apr 89 09:15:49 PDT
Date: Wed, 19 Apr 89 09:14:04 PDT
From: sek%Erin.Caltech.Edu@CitIago.Bitnet (Steve Koonin/P)
Message-Id: <890419091404.23000b0d@Erin.Caltech.Edu>
Subject: Re: The Emperor's Clothing
To: cab%Caltech.Edu%Erin.Caltech.Edu@CitIago.Bitnet, doug@sbitp.bitnet,
nauenbe@ucscd.bitnet, pgs%Caltech.Edu%Erin.Caltech.Edu@CitIago.Bitnet,
rlg2@yktvmt.bitnet

----- Start of forwarded messages -----
Return-path: <perry@ohstpy.bitnet>
Received: from Hamlet.Caltech.Edu by Erin.Caltech.Edu with DECNET ;
Tue, 18 Apr 89 17:11:50 PDT
Received: from Hamlet.Caltech.Edu by caltech.edu via Magic ;
Tue, 18 Apr 89 17:11:17 PDT
Received: from OHSTPY by Hamlet.Bitnet via BITNET ;
Tue, 18 Apr 89 16:43:11 PDT
Received: From OHSTPY(PERRY) by HAMLET with Jnet id 4444
for KOONIN@CALTECH; Tue, 18 Apr 89 16:42 PDT
Message-Id: <890418164311.24600192@Hamlet.Caltech.Edu>
Date: Tue, 18 Apr 89 19:40 EDT
From: <PERRY@OHSTPY>
Subject: The Emperor's Clothing
To: koonin@caltech
Original_To: @FUSION,@FUSION2

The only reference for this message is The Emperor's Clothing. All information given below has been gleaned from conversations with people at various laboratories and from the press. That is to say, everything is still suspect.

To date I know of no reliable confirmation of any aspect of the Pons-Fleischmann work that would indicate fusion. No one else in the U.S. has detected neutrons and the indication of tritium announced at the Univ. of Washington is still premature. This last result can be explained by a number of effects, including simple electrolysis of heavy water containing some tritium. Many laboratories have worked night and day to reproduce the Utah results, and no one has seen anything but background. Excess heat has been seen at Texas A&M, but others seeing heat ascribe it to chemical reactions. In particular, reports from Caltech and Los Alamos indicate that large efforts have led to many physicists and chemists studying background neutrons and gamma-rays, and seeing nothing of interest. In general scientists seem to be remarkably reluctant to criticize the Utah work. Claims that physicists are simply allowing jealousy to cloud their judgement, not wishing to admit that chemists can do what they have failed to do, are absurd.

Several reports indicate that Pons has admitted to seeing excess heat in one cell in which normal water was used instead of heavy water. Pons reportedly admitted this again today at Los Alamos. Draw your own conclusions.

Pons reportedly prevented physicists from the Univ. of Utah from entering his laboratory to check his neutron results and take background counts. No physicist has been allowed in yet, and there are reports that no cells are running at Utah anymore, which should save them from any embarrassment for several weeks of charge-up time.

At Los Alamos today, Pons did not know the purity of his palladium. He says 30% of his cells work. They won't work if the Pd is machined. They won't work if they are wire. They have to be cast. This might make sense, but we can only hope that they work outside the state of Utah.

I wish Feynman were still around. His response to all this would be interesting.
------ End of forwarded messages ------

The beginning of the end?

Andy Brendler

----- FUSION FORUM appended at 11:02:00 on 89/04/19 GMT (by DCA8CMAS at EIHVM01)
Subject: Very Cold Fusion in Italy

Yesterday 17/4/89 ENEA (National Authority for Alternative Power Resources) researchers announced they got VCF from deuterium and titanium (Ti) without applying any electrical potential.
Experimental environment reported today by newspapers:
deuterium: gas state
titanium : chips
temperature: 150K
pressure: 8bar
heat emission: practically negligible
neutron emission: several hundreds/sec
probable reaction: 2D + 2D --> 3He + n
patent: submitted today 18/4/89
chief scientist: Francesco Scaramuzzi
location: ENEA labs in Frascati, Rome
I'm not sure i used proper terminology since i'm only an engineer, not a physicist. Let's wait...
Ciao
Carlo Jacob

Carlo Jacob (DCA8CMAS AT EIHVM01)

(append to disk 'IBMPC')

Date: Tue, 18 Apr 89 12:49 EDT
From: <JBIGELEI@SBCCMAIL>
Subject: Tritium in the Univ. of Utah "FUSION"
To: rlg2@yktvmv
X-Original-To: rlg2@yktvmv.bitnet

State University of New York at Stony Brook
Stony Brook, NY 11794-3400

Jacob Bigeleisen
Professor
Chemistry
516-632-7905
18-Apr-1989 12:42pm EDT

FROM: JBIGELEISEN

TO: Remote Addressee (_FPRESS@NAS.BITNET)
CC: Remote Addressee (_WSPINDEL@NAS.BITNET)
CC: Remote Addressee (_RLG2@YKTVMV.BITNET)

SUBJECT: Tritium in the Univ. of Utah "Fusion"

Dear Frank,

I want you to know that I have serious reservations about the claims of Fleischmann and Pons that they have measured tritium production on their "cold deuterium fusion" experiment. I had prepared a document setting forth my reservations of the observations reported in their preprint to Journal of Electroanalytical Chemistry. Unfortunately, when I went to file the document, my disc crashed. I will try to prepare the document again and send you a copy of my reservations before the end of this week via BITNET.

Jake.

Date: Tue, 18 Apr 89 13:47 EDT
From: <JBIGELEI@SBCCMAIL>
Subject: Jones et. al. 23 March
To: rlg2@yktvmv
X-Original-To: rlg2@yktvmv.bitnet

State University of New York at Stony Brook
Stony Brook, NY 11794-3400

Jacob Bigeleisen
Professor
Chemistry
516-632-7905
18-Apr-1989 01:40pm EDT

FROM: JBIGELEISEN

TO: Remote Addressee (_RLG2@YKTVMV.BITNET)

SUBJECT: Jones et. al. 23 March

Dick:

Do you have a clear legible copy of "Observation of Cold Nuclear Fusion in Condensed Matter" by S.E. Jones, E.P.Palmer..... J. Rafelski dated March 23, 1989. I have a

"xerox" copy of a copy that was sent via FAX from Los Alamos. What is not legible on my copy are the exponents (which are important). If you have a good copy could you bring it to the Academy and I will copy it there. Otherwise I will write to Jones. Their work is a lot more credible than the University of Utah work. JAKE

==========

Received: by NASVM (Mailer X1.25) id 4890; Tue, 18 Apr 89 22:27:31 EDT
Received: by NAS (PC Mail Gateway) id 6031; Tue, 18 Apr 89 22:27:30 EDT
Date: Tue, 18 Apr 89 21:55 EDT
From: Frank Press <FPRESS@NAS>
Subject: Undelivered mail
To: <RLG2@YKTVMV>

Received: by NASVM (Mailer X1.25) id 4708; Tue, 18 Apr 89 16:43:59 EDT

Your mail was not delivered to some or all of its
intended recipients for the following reason(s):

Unknown domain: VMV ...

----------RETURNED MAIL FILE----------
Received: by NASVM (Mailer X1.25) id 4707; Tue, 18 Apr 89 16:43:58 EDT
Received: by NAS (PC Mail Gateway) id 6021; Tue, 18 Apr 89 16:43:53 EDT
Date: Tue, 18 Apr 89 16:18 EDT
From: Frank Press <FPRESS@NAS>
Subject: Cold Fusion
To: <RLG2@YKT.VMV>

Please call Buford Price. He is arranging for two papers at the Class I session at the annual meeting. Give him your views about appropriateness of concept and also his choice of speakers. I am on the road and would appreciate your input. Buford is doing this as Chair of Physics section. Thanks for your report. Could you send a copy to Bill Press at WPRESS@CFA. My software doesn't allow me to forward bitnet messages from a laptop.

Received: by talcott.harvard.edu; Tue, 18 Apr 89 20:04:37 EDT
Received: by cfa; Tue, 18 Apr 89 20:05:30 edt
Date: Tue, 18 Apr 89 20:05:30 edt
From: wpress%cfata2.DECNET@harvunxw.BITNET (WPRESS@CFA wpress@cfa.harvard.edu 66
98::WPRESS)
To: cfairt::rlg2@yktvmv.bitnet
Subject: Dick, this may have been misaddressed originally. Over to you.

From: CFAPS2::"FPRESS@NAS.BITNET" 18-APR-1989 18:58
To: CFATA2::WPRESS
Subj: Cold Fusion and geophysical anomalies

Received: from NASVM.BITNET by cfa.bitnet with BSMTP via BITNET ;
Tue, 18 Apr 89 18:58:55 EDT
Received: From NASVM(MAILER) by CFA with RSCS id 4802
for MAILER@CFA; Tue, 18 Apr 89 18:58 EST
Message-Id: <890418185856.02m@cfaps2>
Sender: MAILER@NASVM.BITNET
Received: by NASVM (Mailer X1.25) id 4799; Tue, 18 Apr 89 18:56:02 EDT
Received: by NAS (PC Mail Gateway) id 6028; Tue, 18 Apr 89 18:55:58 EDT
Date: Tue, 18 Apr 89 18:27 EDT
From: Frank Press <FPRESS@NAS>
Subject: Cold Fusion and geophysical anomalies
To: <RLG2@YKTVNV>,
<WPRESS@CFA>

Your article refers to these as does Jones inhis paper. The observed geothermal heat flow is easily accomodated by the measured abundances of U,K,and Th isotopes in crustal granitic and mantle ultrabasic rocks. In fact only 20 km or so of granite is needed to account for all of the heat flow on continents. The heat flow on the sea floor is nicely explained by a combination of the radioactive elements in ultrabasic rocks in the mantle, and the variation of heat flow across the sea floor is what is expected for plate creation and cooling by sea floor spreading. This is well known stuff. The tritium measurements referred to in Jones' paper could be leftover tritium from nuclear weapons tests circulating in meteoric water. It has been observed in many places and is used to measure the circulation time in different types of hydrological cycles.

Dick - I misaddressed an earlier message to you. I would appreciateit if you call Buford Price at Berkely. He is chair of Physics section and wants to organize a cold fusion session at the Class I meeting at the annual meeting. Give him your views about the concept and about the speakers. I am on the road. Thanks.

==========

Date: 18 April 1989, 15:10:26 EDT
From: R.L.Garwin (914) 945-2555 RLG2 at YKTVMV
IBM Fellow
and Science Advisor to the Director of Research
T.J. Watson Research Center, P.O. Box 218
Yorktown Heights, NY 10598
To: KOONIN at SBITP
cc: RAMULLER at LBL
Prof. William Press W: (617) 495-4908 WPRESS at CFA2
Subject: Cheves Walling at Utah.

NYT of 04/18/89 has Walling and Simon(s) of UU giving a theory of D + D going to He-4 without neutron or proton emission. I have just talked with Walling, who will send me a FAX of their paper.

They reason as follows: Can get fusion rate to yield observed excess heat if electron m* = 10 or so. With electrons of such m*, internal conversion of energy from the excited state of the compound nucleus is increased ("Fowler theory") by m* to the 5'th power. Only comparison they could find was with excited state of carbon, which had decay rate of 10**9 per second .

I pointed out problems with believing the m* of solid state physicists has anything to do with shielding at such close distances, or with internal conversion, etc.

Experimental claim: they have a high resolution mass spectrometer and feed the off gas from the cells producing excess energy to it. They see the D2 peak at M=4 and the neighboring He4 peak at M=4-. They see about 10**12 He4 yield per second!

Well, I don't for a minute believe that the He4 deexcites by I.C., and if it did there would be hard x-rays. But IS something going on? Why doesn't mu- catalyzed fusion produce MeV muons rather than the usual reaction products?

Presumably I can go from He4 ground state (or excited state) to separated deuterons by electromagnetic interaction alone, as in photo-ionization of atoms to the continuum. Of course from the ground state this must be electric quadrupole, but could be electric dipole to (from) an excited state. BUT, if I try to do this from the low-energy deuterons in the solid, I have the mismatch in wavelength between gamma ray and deuterons. Using electrons instead of gammas doesn't improve things much.

Think of something.

Dick Garwin

==========

Date: 18 April 1989, 14:13:10 EDT
From: R.L.Garwin (914) 945-2555 RLG2 at YKTVMV
IBM Fellow
and Science Advisor to the Director of Research
T.J. Watson Research Center, P.O. Box 218
Yorktown Heights, NY 10598
To: FPRESS at NAS
Cc: STEINH at PENNDRLS
KOONIN at SBITP
RAMULLER at LBL.GOV
JSL at ASPL **wrong address, but JSL at CASP1 doesn't work any better**
JBIGELEI at SBCCMAIL
KNEIFEL at SBCCMAIL

Subject: Cold nuclear fusion.

Frank, I helped arrange a day-long session in Erice, Sicily, last Wednesday and participated with Fleischmann, Jones, Denys Wilkinson, and about 100 others. Here is a report to be published in Nature in a few days. Please note the extreme skepticism about the claims of observation of fusion heat.

Dick Garwin
****
April 16, 1989 23:27 New York time.

****

An International Forum on Cold Nuclear Fusion*

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* April 12, 1989, at "Ettore Majorana" Center for Scientific Culture, Erice, Sicily, convened by Antonino Zichichi, Director.

_____
began with presentations by M. Fleischmann&s1.

_____
1. Fleischmann, M...., J. Electroanalytical Chemistry, ...

_____
of the work at the University of Utah, and by S.E. Jones and J.B. Szirr&s2.

_____
2. Jones, S.E.,.... ***03/23/89 submitted to NATURE..***

_____
of their work at Brigham Young University, Provo, Utah, on room temperature nuclear fusion of deuterium-- cold nuclear fusion. A full day of presentations and discussion brought no consensus on the results, no credible theory to explain them, but some suggestions as to where to look for explanation or confirmation.

FAMILIAR REACTIONS IN UNEXPECTED CIRCUMSTANCES?

Jones, et al, electrolyze&s2. heavy water (99.5% D&sub.2&sup.O) in a pH 3 solution of a witch's brew of salts (including Li and Pd), with rough Ti chunks or Pd foil as cathode, driving deuterium into the metal with a supply voltage of 3-25 V at cell currents of 10-500 mA. A gold foil was used as anode in each 20-ml cell. A counter designed to detect and identify fast neutrons indicates a total of 170 +/- 23 counts with a pulse-height spectrum consistent with that expected for the 2.45 MeV neutron of the well-known fusion reaction:

d + d = &s3.He (0.82 MeV) + n (2.45 MeV) (i)

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J.B. Czirr described the neutron detector in detail, which detects a thermalized neutron by the light flash in

&s6.Li-doped glass and determines the neutron energy by the overall fast light pulse emitted in the preceding 20 microseconds from a liquid scintillator in which the proton recoils occur as the neutron thermalizes. The counting rate is but 2 per hour in the relevant region of pulse height.

Analogous experiments&s1. used a strongly alkaline solution of 0.1 M LiOD in heavy water to drive deuterons into Pd rod cathodes (cast and machined) under the influence of cell currents up to 800 mA and voltages typically of 12 V. Reaction (i) is to be detected by the 2.22 MeV gamma ray that results from capture of the 2.45 MeV neutron (after thermalization) on a proton of the surrounding water bath:

p + n = d + &gamma. (2.224 MeV)

(ii)

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A very narrow peak at 2.2 MeV containing some 3000 &gamma.-ray counts is shown for an NaI scintillation detector close to the electrolytic cells, in comparison with the "level spectrum" in a similar detector 5 m or 10 m away. Unfortunately, the full pulse-height spectrum is not shown, so one can't verify the presence of the annihilation radiation "escape peaks" that would lend more confidence to the neutron origin of these counts. Furthermore, no evidence connects the counts with current applied to the cell, and there have been no runs with ordinary water as a control.

The scientific and patent literature for the last 60 years have occasional claims of nuclear fusion catalyzed by Pd, but there have previously been no credible reports of neutrons from metal deuterides for reasons thought to be well understood: the Coulomb barrier to close approach by nuclei of charges Z&sub.1&sup.e and Z&sub.2&sup.e amounts to (Z&sub.1&sup.e)x(Z&sub.2&sup.e)/r, or about 600 keV for Z&sub.1&sup.=Z&sub.2&sup.=1, reducing the quantum mechanical tunneling probability to a very small value for deuterons in a D&sub.2&sup. molecule to approach to with the range of nuclear forces. New calculations were presented to the Forum&s3. (Koonin, Caltech)

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3. Koonin, S.E., and Nauenberg, M., (submitted to Nature 7 April, 1989)

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of the fusion reactions per second per deuteron &lambda.&sub.f-dd&sup. bound in a deuterium molecule by "electrons" of normal charge but mass m* instead of m&sub.e&sup.:

.sk;.fo off

.tp 13 23 33 53 53

Table 1. Base-10 logarithms of fusion rates per deuteron per second.

m*/m&sub.e&sup.#1#2#5#10#

&lambda.&sub.f-dd&sup.#-63.5#-40.4#-19.8#-09.1#

&lambda.&sub.f-pd&sup.#-55.0#-36.0#-19.0#-10.4#

.sk;.fo

Other participants agreed with these results, which correct an error in some previous calculations and use a more accurate molecular potential. An important experimental point is provided by the m*=207 case, in which a negative muon binds two deuterons as a molecular ion 207 times smaller in dimension than the normal molecular ion, with &lambda.&sub.f-dd&sup. of some 10&sup.9&sub. s&sup.-1&sub. as predicted&s3.

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3. F.C. Frank, Nature, V. 160, 525 (1947).

4. L.W. Alvarez, et al., Phys. Rev., V. 105, 1127 (1957).

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and observed&s4..

The results of Table 1 show the strikingly easier penetration of the barrier by the proton (because of the "reduced mass" = M&sub.1&sup.M&sub.2&sup./(M&sub.1&sup.+M&sub.2&sup.) in the barrier penetration factor

B = e&sup.-2&integral.k(r)dr&sub., where

k(r)=[2&mu.(V(r)-&epsilon.)]&sup.1/2&sub. and the integral runs from 0 to the classical turning point r&sub.0&sup..
In the region of the claimed neutron production rate&s2., &lambda.&sub.f-dd&sup.=10&sup.-20&sub., so that m*/m&sub.e&sup.=5 from Table 1. A similar value is needed to explain the &gamma.-ray counts&s1..

Although quasi particles of high effective mass are well known in metals, their m* relates to density of states vs. energy in the band structure of the lattice; a quasi particle of m*=5 is not capable of binding two deuterons to a density 5&s3.=125 times that of molecular hydrogen, or of allowing the nuclei to approach one another to a distance 5 times smaller than the 0.74 &Angstrom. internuclear separation in D&sub.2&sup..-- 0.15 &Angstrom., at which the repulsive potential amounts to some 95 eV.

It was suggested therefore to look for dynamic effects to augment the equilibrium tunneling-- phonon-assisted tunneling (Koonin), or coherent acceleration (Ponomarev, USSR) in which traveling electron density waves could trap deuterons and accelerate them to the same velocity as the waves; the deuteron kinetic energy would then be some 3700 times that of an electron of the same velocity and would greatly enhance the tunneling through the barrier. Alternatively, a solution could be sought in "high-T&sub.c&sup. superconductivity or other miracle of solid-state physics."

Several experimental groups at the Forum presented results showing no neutrons or gamma rays generated in replication of (1) or (2). Electrolyzing 1-mm by 10-cm Pd rods for 10 days gave neutron yields below 0.6 s&sup.-1&sub.-cm&sup.-3&sub. (M.M. Broer, AT & T Bell Labs), (less than 10&sup.-3&sub. that of that reported in similar circumstances&s1.) and an upper limit of 10&sup.-3&sub. s&sup.-1&sub.-cm&sup.-3&sub. was set for the detection of t or n from the d+d reaction (J.E. Ziegler, IBM). Experiments were reported (Celani, Frascati) with "some increase in neutron signal at the beginning of each experiment for about 5 minutes, but indistinguishable from background after 20 minutes." Experiments will be transferred to the great underground laboratory at Gran Sasso; perhaps also those who claim the ability to produce neutrons will be hospitable to those more adept at detecting than at producing them in this way.

NON L'AVREI GIANMAI CREDUTO...
MA FARO QUELCHE POTRO...
("I would never have believed this, but I'll see what I can do,"
Mozart's Don Giovanni,
quoted by Maiani??)
NATURE should decide if it wants to use this quotation. If so, you or I will have to get the precise wording

This, of course, refers to the heat generation claimed&s1. of some 10 W per cc for 100 hours or more, as well as destructive releases of heat that fuse and vaporize Pd and destroy the cell. Although the latter are likely to result from the electrochemical creation of high explosive by stuffing hydrogen into high-energy sites in Pd (analogous to the Wigner energy in neutron-irradiated graphite), no such explanation can account for the 4 MJ/cc or 600 eV per atom to which such continuous energy release would correspond, if indeed there is such excess heat flow to the surrounding water bath, in excess of that represented by the product of current and voltage applied to the cell (typically 0.8 A at 12 V). Stored energy could be at most 3 eV or so in any chemical reaction, so it is of the utmost importance to inquire into the details of this measurement; unfortunately, details are lacking.

The "excess enthalpy generation" is measured calorimetrically&s1. by a "calibrated thermistor" as a &Delta.T between the contents of the electrolytic cell and a surrounding thermostated water bath, in

comparison with the &Delta.T measured for a resistance heater in the cell itself; the thermal impedance is that posed by the dewar flask in which each experiment is conducted. For rods of 1 mm, 2 mm, 4 mm diameter, an excess heat rate is found&s1. that depends strongly on the current density, amounting to 8-20 W/cc at 0.5 A/cm&s2., and that the excess heat persists during the operation of the cell for hundreds of hours, even though the surface of the Pd rod darkens.

I have seen insufficient evidence to believe that there is "excess heat," since the &Delta.T is measured between the bath and the thermistor in the electrolytic cell, rather than along a fixed conductive link between cell and bath. Specifically, if there are significant temperature gradients within the cell because of imperfect stirring or local recombination of hydrogen and oxygen gas, the thermistor temperature will not be the temperature of the inside wall of the flask, resulting in very substantial errors in inferred heat generation.

Even more striking than a heat-producing fusion reaction at a rate some 6x10&s1.&s2. cm&sup.-3&sub. s&sup.-1&sub. is the simultaneous claim of detection of only 4x10&s3. neutrons produced cm&sup.-3&sub. s&sup.-1&sub.; fewer than 10&sup.-9&sub. of the reactions are supposed to produce a neutron. How can this happen? A coherent superposition of isotopic spin states for d+d could cancel the neutron-producing reaction and reinforce the t+p branch (D. Wilkinson, ??), but would not eliminate the usual isospin-zero reaction; it would thus change the neutron branching ratio only if the barrier penetration were greatly facilitated. This effect can be estimated, and I judge the effect is only a few per cent rather than a factor 10&s9..

Suggestions of radiationless de-excitation of the &s4.He intermediate state formed by d+d thus far fail in two regards: first, the lack of a mechanism, and second, because such a mechanism would
.us add
a channel to the usual particle decay of &s4.He rather than suppress the usual channel. Thus such a mechanism would need to be 10&s9. times faster than the usual particle channels that themselves occur in nuclear transit times-- a totally new phenomenon. Finally, the needed mechanism must not have shown itself in the measurement of the cross sections-- some of which were done in gas cells, but some, at times, in metal hydrides.

"SOMEBODY IS GOING TO EAT HIS HAT" (Maiani, ??)
"WE ARE ALSO HUMAN, AND NEED MIRACLES, AND HOPE THEY EXIST" (Ponomarev, USSR).

A few neutrons each second (or a few thousand) from an electrolytic cell may be cold nuclear fusion, or may be due to an "arcs and sparks" origin. Within the next few weeks, experiments will surely show whether cold nuclear fusion is taking place; if so, it will teach us much besides humility and may indeed provide insight into significant geophysical puzzles&s2.. Large heat release from fusion at room temperature&s1. would be a multi-dimensional revolution; I bet against its confirmation.

Richard L. Garwin
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Two Innocent Chemists Look at Cold Fusion

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Abstract

We propose that the large energy release reported in the experiments of Fleischmann, Pons, and Hawkins is the consequence of ^2H fusion accelerated through screening by neighboring "heavy electrons" with mass $m^* \cong 10$ electron masses. Such effects are proposed to also accelerate the internal conversion (IC) of the transient excited $^4He^*$ by a factor of 10^2 to 10^8. If IC exceeds the rate of the usual fragmentation of $^4He^*$, this can explain why the bulk of the energy is released as heat rather than via neutron and tritium production. Our analysis also suggests that the rate of $^1H + ^2H$ fusion should be some 600 times that of $^2H + ^2H$ fusion in the Pd lattice, so that fusion might even be observed in water of natural-abundance deuterium content.

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Recently, Fleischmann, Pons, and Hawkins[1] have reported that the prolonged electrochemical charging of a palladium cathode with deuterium by electrolysis of a D_2O solution leads to the evolution of neutrons and tritium together with heat on a scale not easily reconciled with any known chemical reaction. This report has attracted world-wide attention and no little skepticism, although, a this time, there appears to have been piecemeal confirmation[2] of their results in a number of laboratories.

The evolution of tritium and neutrons is consistent with the well-known fusion of deuterium to form an excited helium nucleus with approximately 24 MeV excess energy

$$2\,{}^2H \Rightarrow {}^4He^* + 23.85\ MeV \qquad (1)$$

which, under conditions studied by physicists, decomposes to roughly equal quantities of ^3He and ^3H

$$^4He^* \Rightarrow {}^3He + n + 3.25\ MeV \qquad (2)$$

$$^4He^* \Rightarrow {}^3H + {}^1H + 4.03\ MeV \qquad (3)$$

The most plausible explanation of the apparent enhanced rate of $^2H + {}^2H$ fusion involves a tunnelling phenomenon aided by screening of the coulomb repulsion between the $^2H^+$ ions by the surrounding electron cloud[3] in the lattice as discussed further below. The conventional reaction sequence (1-3) produces a predictable amount of heat, based upon that generated in the neutron- and ^3H-producing reactions. The most startling feature of the experimental results of ref.(1) is that the actual heat production, measured by simple calorimetry, is 10^7- 10^{10} as large as that expected from the above reaction sequence. Clearly, if these experiments are correct, the major path of energy production is something quite different.

At the molecular level with which chemists are familiar, electronically excited states of molecules are known to lose their energy by at least three well-recognized paths: A) dissociation of the molecule by the breaking of chemical bonds; B) emission of light (fluorescence or phosphorescence); C) radiationless transfer of energy to surrounding molecules, usually as vibrational energy, but sometimes by converting them to electronically excited or ionized states. Analogs of each of these are known for the decay of excited

nuclei. Reactions (2) and (3) clearly parallel path A. The analog of path B is γ- ray emission; this was not happening in the experiments of ref.(1) since it would have carried most of the energy out of the reaction vessel, and the resulting lethal level of radiation would have been detected by radiation monitors in the laboratory. The analog of path C is known[4] as internal conversion (IC) in which energy is transferred from the exctied $^4He^*$ nucleus by coupling to neighboring electrons (of the solid's bands) which are ejected as β-radiation, most of whose kinetic energy would eventually produce heat within the palladium electrode

$$^4He^* \Rightarrow {}^4He + \beta\ (\sim 24\ MeV) \qquad (4)$$

Our proposal that the IC path may be involved is certainly attractive from the point of view of heat and energy production, since it predicts that each fusion event will produce approximately 24 MeV, rather than 3.25- 4.0 MeV of energy, unaccompanied by a large, troublesome neutron flux or by ^3H formation. Experimentally, it demands a rather copious production of ^4He in a reaction generating 10 watts cm^{-3} of excess energy (in the range which has been observed[1]) or some 2.6×10^{12} fusions $cm^{-3}\ sec^{-1}$.

Pons and Hawkins have informed us[5] that, in a preliminary experiment, mass spectrometric analysis of evolved gases from a cell operating at 200 milliamps with an electrode volume of 0.0785 cm^3 and delivering 0.5 watts cm^{-3} of excess heat showed a $^4He/D_2$ ratio of 10^{-5} to 10^{-6} and substantially larger than that of a number of blank determinations. This corresponds to 8×10^{11} to 8×10^{12} atoms of $^4He\ cm^{-3}\ sec^{-1}$, certainly of the same order of magnitude as (actually larger than) the value of 1.3×10^{11} $^4He\ cm^{-3}\ sec^{-1}$ obtained by scaling the number 2.6×10^{12} calculated above by the excess-heat ratio 0.5/10.

As far as we know, internal conversion has not previously been observed in deuterium fusion reactions or in the decay of other $^4He^*$ states. Clearly, the question is why might it be occurring in the experiments of ref.(1)? In the model described here, which is based on an extension of the conventional picture of internal conversion described in the physics literature[4], it is demonstrated that the same electronic screening phenomenon which may lead to accelerated

fusion should also greatly increase the rate of internal conversion of the resulting $^4He^*$ to an extent that IC may compete with the usual fragmentations (2) and (3).

To describe the fusion of two $^2H^+$ nuclei, we use a model in which the rate R (in fusion events per sec per $^2H^+$) is given as a collision frequency f multiplied by a probability of fusion: $R = f \times P$. In characterizing the state of the deuterium in the Pd metal, it is assumed that the mole ratio of $^2H^+$ to Pd is nearly one-to-one. In fully saturated Pd, the ratio lies between 0.5 and 1.0; in what follows, a value of 0.7 is assumed, giving a $^2H^+$ density of 4.8×10^{22} $^2H^+$ cm^{-3}. This concentration implies an average spacing between $^2H^+$ ions (if they were uniformly distributed) of 3.4Å. If the temperature in the Pd lattice ranges from 300 °K to 1000 °K , the mean collisional velocities of the $^2H^+$ pairs would be $(2.7 \text{ to } 5)/\mu^{1/2} \times 10^5$ cm sec^{-1}, where μ is the reduced mass, in amu's; the nominal value of $4\ \mu^{-1/2} \times 10^5$ cm sec^{-1} chosen here corresponds to a collision frequency of $f = 1.3\ \mu^{-1/2} \times 10^{13}$ sec^{-1} (for each $^2H^+$).

The fusion probability function P is taken to be of the form[3] $P = \exp(-2\pi\alpha/\hbar v)$, where v is the relative speed of the colliding $^2H^+$ species, and the parameter α characterizes the strength of the coulombic repulsion between the two nuclei. To determine α for the ^2H - ^2H interaction, the following procedure is used:

1. The expected[6] fusion rate of isolated D_2 of $10^{-63.4}$ sec^{-1} is used, in conjunction with a "collision frequency" of 7×10^{13} sec^{-1} (the vibrational frequency of D_2 is used in this case instead of the thermal collision frequency) and the corresponding velocity of 7×10^5 cm sec^{-1} and a reduced mass μ of 1.0 amu, to determine the value of $\alpha = 2.08 \times 10^{-20}$.
2. From this "fit", it follows that $P = \exp(-178\ v/v^*)$, where v^*/v is the ratio of the collision speed in any particular situation and the speed 7×10^5 cm sec^{-1} used in determining α.

The physical significance of α can be made clear by recalling (see Eqs.(9) and (10) of ref(3)) that the functional form given above for P is appropriate for tunnelling, from $r = \infty$ to $r = 0$, of a species having velocity v and reduced mass μ through a coulomb barrier. The true nuclear potential involves such a coulomb repulsion (e^2/r) , only

for values of r greater than the nuclear size (r_n x10^{-12} cm). To correct for the use of the coulomb-type potential in the tunnelling equation between r = 0 and r = r_n x10^{-12} cm, one introduces the factor α. In effect, α describes a coulomb potential that has been scaled to yield a lower effective potential which is then used for al r-values.

The above form for P and the collision frequencies $f = \mu^{-1/2}$ 1.3 x10^{13} cm^{-3} sec^{-1}, permit the log of the fusion rate R (in sec^{-1}) to be written as:

$$-\log R = 13.8 - 77 (v/v^*) .$$

To achieve a rate of $^4He^*$ formation equal to 2.6 x10^{12} cm^{-3} sec^{-1} as inferred earlier, given the concentration of $^2H^+$ to be 4.8 x10^{22} $^2H^+$ cm^{-3}, requires a fusion rate per $^2H^+$ of 5.4 x10^{-11} fusions sec^{-1} or a lifetime of 585 years. Using the log of this rate in the above rate expression, yields $v^*/v = 3.3$.

As described on p.218 of ref(3), the effective collision energy E, which in the absence of any metallic screening would be equal to the thermal energy, can be increased because the surrounding sea of electrons in the metal serve to screen the repulsive coulombic potential between the two $^2H^+$ nuclei. This same energy lowering can be modelled in terms of the "binding" together of the two $^2H^+$ ions due to[6,7] "heavy electrons" in the metal. In either model, the collision energy E of the $^2H^+$ pair is increased by a factor m* equal to the ratio of the metal's effective electron mass and the bare electron mass: $E^*/E = m^*$. Considering this increase in energy, which yields a speed ratio $v^*/v = m^{*1/2}$, and realizing that $v = (2E/\mu)^{1/2}$, allows the fusion rate expression to be extended to treat events taking place in the presence of screening for which:

$$-\log R = 13.8 - 77 (\mu^{1/2} / m^{*1/2}) . \qquad (5)$$

In terms of the model introduced here to "explain" cold fusion in Pd metal, the rate acceleration required to account for the production of 10 watts/cm^3 requires an effective electron mass of m* = 10.

This same model can be used to predict the rates of other fusion events that might occur in the Pd system. In particular, using the following estimated rates[6] of fusion: $^1H + ^2H$ (10^{-55} sec^{-1}), ^1H +

^3H (10^{-58} sec^{-1}) and $^2H + ^3H$ (10^{-69} sec^{-1}) in the absence of screening to determine α- values for each reaction, and employing an effective electron mass of $m^* = 10$, the above model predicts rates of $10^{-7.5}$, $10^{-8.7}$, and $10^{-12.5}$ sec^{-1}, respectively, in the Pd metal. Although these rate estimates should be taken as rather uncertain, primarily because of uncertainty in knowledge of the α parameters used in the tunnelling formulas, they suggest that the fusion of $^1H + ^2H$ ($10^{-7.5}$ sec^{-1}) may be important, but that $^1H + ^3H$ and $^2H + ^3H$ fusion are probably not (because of the abundances of ^1H, ^2H and ^3H). They also suggest that, in ordinary H_2O, where D_2O occurs at 0.015% in natural abundance, the $^1H + ^2H$ fusion may take place at appreciable rates because of the $^1H + ^2H$ to $^2H + ^2H$ rate ratio of $10^{-7.5}$ to $10^{-10.3}$ = 631. It clearly also indicates that mixtures of D_2O and H_2O might yield even higher energy production ($^2H + ^1H \Rightarrow ^3He + \gamma$ (5.6 MeV)) if the internal conversion process described below were also operative for $^3He^*$. Although the energy per $^2H + ^1H$ fusion is only 23% of that involved in $^4He^*$ decay, the 631-fold increase in fusion rate could yield a much larger energy production rate if the above estimates are accurate. The "ideal" H_2O/D_2O mole fraction can be calculated and depends on the fusion rate ratio and the energy per fusion ratio (631 and 0.23, respectively, according to our estimates). Finally, the m^* dependence of log R expressed in Eq.(5) suggests that a search be undertaken for materials and/or conditions which permit high ^2H and/or ^1H concentrations to be established and which provide, through the lattice band structure, even larger m^* values; such materials could yield even larger energy production rates.

Because IC rates scale strongly with the electron density near the nucleus from which they receive energy, and because this density should be greatly enhanced for electrons of mass $m^* = 10$, our model also raises the possibility that IC could become so rapid as to swamp out the usual fragmentation of $^4He^*$, with the consequence that the bulk of the fusion energy would be liberated as heat as is apparently observed.

Using the absolute rate of 4 $x10^{10}$ sec^{-1} obtained by Fowler[4] for IC in Ra (Z=84) and scaling (see Eq.(47) of ref.(4)) by the fourth power of the nuclear charges $(2/84)^4 = 3.25\ x10^{-7}$ and the fifth power of the electron mass $(m^* = 10)^5 = 10^5$, gives an IC rate estimate for $^4He^*$ of 1.3 $x10^9$ sec^{-1}. It is not known (to us) what the absolute rates of fragmentation of $^4He^*$ to $^3He + n$ or $^3H + {}^1H$ are in the Pd lattice (rates of fragmentation of somewhat heavier nuclei (not in a lattice) of 10^9 sec^{-1} are mentioned by Fowler[4]). Clearly, if the $^4He^*$ fragmentation rates are less than1.3 $x10^9$ sec^{-1} , little neutron or tritium signal will be detected. This IC-based model also predicts that ^4He should be formed at rates[5] of 2.6 $x10^{12}$ cm^{-3} sec^{-1}, that IC-generated β-emission should occur, and that the neutron and tritium yield should decrease as the rate of fusion increases; all of these predictions can and should be tested.

Although our IC rate estimate is rather crude and is highly sensitive to the value used for m*, it serves to illustrate that IC may indeed play a role in the conversion of $^4He^*$'s excess energy to heat. The α parameter used in the tunnelling calculation can be varied by at most a factor of 2 while still maintaining agreement with what is thought[6] about the rate of fusion of D_2. Such a variation would be reflected in a range of m* values of 3 to 40, which, in turn, would lead to IC rates between 3 $x10^6$ and 1.3 $x10^{12}$ sec^{-1} . The fifth power dependence of the IC rate on m* causes, in our opinion, these rates to be the least accurate of our predictions.

In summary, we propose that the high rate of energy production observed by Fleischmann, Pons, and Hawkins[1] arises from ^2H fusion via a tunnelling process facilitated by shielding of the coulombic repulsion between $^2H^+$ nuclei by neighboring electrons in the PdD_x lattice acting as "heavy electrons", and that this same shielding facilitates internal conversion of $^4He^*$ so that the bulk of the energy is detected as heat. We further suggest that fusion of ^2H and ^1H is accelerated by this shielding and might be an even more rapid process. Finally, we believe that, even if our analysis is incorrect in detail, any model interpreting accelerated fusion as due to electronic shielding will predict a parallel increase in the rate of internal conversion of the excited nuclei formed.

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1. M. Fleischmann, S. Pons, and M. Hawkins, J. Electroanal. Chem. 1989 261, 301.

2. At this time, based on news reports and private communication.

3. Harrison, E. R., Proc. Phys. Soc. 1964 84, 213 deals with the concept of enhanced tunnelling due to screening by electrons in the surrounding medium for so-called pycnonuclear reactions. He also presents the tunnelling formula used here.

4. Fowler, R. H., Proc. Roy. Soc. 1930 129, 1 provides one of the earliest accounts on the internal conversion process which now appears in most texts on nuclear physics.

5. B. S. Pons and M. Hawkins, private communication to the authors.

6. In April of 1989, we received a reprint entitled Cold fusion in molecular hydrogen by Koonin, S. E. and Nauenberg, M. from Professor S. Pons. In this paper, natural-condition fusion rates for D_2, HD, HT, and DT are estimated and enhanced fusion rates are computed and attributed to "heavy electrons" using a method which differs somewhat from ours. No mention of internal conversion or any other mechanism for dissipating the $^4He^*$'s excess energy as heat is made in this preprint.

7. The concept of an effective electron mass is well established in solid-state physics; it is discussed, for example, in the text Quantum Mechanics by A. S. Davydov, NEO Press, Ann Arbor, Michigan (1966).

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Research Report

Electrochemical Experiments in Cold Nuclear Fusion

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ELECTROCHEMICAL EXPERIMENTS IN COLD NUCLEAR FUSION

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Recently, two scientific papers have reported positive detection of nuclear radiation from similar electro-chemical cells operating with deuterated water. Fleischmann and Pons have observed gamma rays at 2.2 MeV at a rate of about 4000/cm^3 – sec, and if the heat they observe is due to unobserved nuclear fusions, they have a fusion rate of about 10^{12} fusions/cm^3 – sec (the subscript "cm^3" refers to the volume of Pd cathode used). In an independent work, Jones et al. have reported detecting 2.4 MeV neutrons at a rate of 0.7 neutrons/cm^3 – sec from an electrolytic cell.

We have experimented with similar electrolytic cells and have looked for energetic charged particles which are characteristic of nuclear fusion reactions. We report on six variations of the cell, with an upper limit of 0.05 detected particles/cm^3-sec. Within background statistics, we observe zero nuclear fusions.

(Submitted to Physical Review Letters: 18 April, 1989)

INTRODUCTION

Two recent papers have created wide-spread interest in Cold Nuclear Fusion (CNF) by reporting the detection of nuclear reaction products from similar electrolytic cells. Fleischmann and Pons (1) have reported the observation of excess gamma rays, neutrons and ^3H from a cell with an anode of Pt, a cathode of Pd and an electrolyte of heavy water, D_2O, with LiOD. Their gamma ray spectrum has a pronounced peak at 2.2 MeV, which they suggest may be from a n+p nuclear reaction (the electrolytic cell is surrounded by a H_2O moderator and neutrons from CNF d+d reactions may interact with the protons in the water). Further, they report massive and sustained heat generation -- of the order of $4MJ/cm^3$. This heat cannot be accounted for by chemical means, so the conclusion they reach is that the heat must be due to CNF processes of an unknown type. The heat produced was observed to scale with the Pd cathode volume, so the CNF is presumed to occur throughout the cathode metal.

Jones et al. (2) have reported the observation of neutrons during a similar electrolytic experiment, with an anode of Au, a cathode of Pd or Ti, and a complex electrolyte of various metallic salts (chosen to represent typical components of the Earth's crust) in heavy water, D_2O. Their neutron spectrometer shows a peak at about 2.5 MeV, exactly the energy of the neutron produced by a d+d nuclear reaction.

In both of the above experiments there was minimal reporting of material analysis before or after the operation of the cells. No mention was made of contamination of the electrolyte from, for example, chemical erosion of the electrodes or by the corrosive electrolyte of the cell walls. Neither group reported the properties of the electrolyte after the cell operation. Both groups have discussed an "incubation period" of charging the cathode, possibly to raise the deuterium within the Pd cathode to high levels. They both conclude that the detected nuclear products were due to d+d reactions, although their data does not rule out other fusion reactions.

Both of these works reflect a long scientific interest in cold nuclear fusion which dates back to 1926 (3). A successful demonstration of CNF could have a profound effect on man's future.

Our approach has been based on the fact that all exothermic cold nuclear fusion combinations have one product in common: energetic charged particles. The above reports have looked at gamma rays, neutrons and excess ^3H -- all of which have large backgrounds in most laboratories. However, energetic charged particles have a background only from naturally occurring alpha particles and cosmic rays. Both of these can be reduced to levels of less than five detected particles per day, a level about 10^{-6} less than that usually reported for the detection of gamma rays or neutrons. Further, the detection efficiency of particle detectors, such as silicon surface barrier detectors (SSB detectors), is about 100% for all energetic incident charged particles in contrast to efficiencies of less than 10% for most gamma or neutron spectrometers.

The charged particles from d+d fusion reactions range from 1 to 3 MeV. We have constructed electrolytic cells with Pd cathodes which directly cover SSB detectors, see Figure 1. Ref. (1) has shown that CNF scales with the volume of the Pd cathode. Hence any fusions which occur close to the back surface of the cathode may produce particles which can be detected by the SSB detectors. Since one d+d nuclear reaction product is an energetic proton (3.02 MeV) which has a range in Pd of about 30 μm, then Pd cathodes thinner than 25 μm will allow most of these emitted in a backward direction to be detected.

Our cells have SSB detectors with a solid angle of about one π and a background of about 5 counts/day (for 1 - 3 MeV particles).

IBM RESULTS

SAMPLE	D_2O/H_2O	TIME	FUSION RATE (FUSIONS/cm^3-sec)
F&P (γ)	100/0		4000
F&P (Heat)	100/0		10^{12}
JONES (n)	100/0		0.7

			PARTICLE DETECTION RATE ($1/cm^3$-sec)
Pd foil (α)	100/0	217	<.0044
Pd sheet (β)	90/10	71	<.00092
Pd foil (Pre Baked)	100/0	217	<.0051
Pd foil	0/100	217	<.0042
Pd + ^6Li (Diff)	90/10	127	<.0066
Pd + ^6Li (II)	90/10	70	<.0096

2σ

EXPERIMENTAL PROCEDURE

The electrolytic cells are shown in Figure 1. Each cell is made from 8 cm diameter cylindrical teflon with a hollow cylindrical well 5 cm in diameter by 8 cm deep. A $7cm^2$ conical hole in the inner surface of the cell leads to a 2 cm^2 Pd cathode. A surface barrier detector faces against the back of the cathode.

The anode is made of Pt (99.9%), 2 cm wide by 4 cm long, with a thickness of .25 mm. Its distance from the cathode may be varied, but usually it is kept at a distance of 2 cm (as in Ref (2)). A thermocouple was attached to the top of the Pt anode to determine approximate electrolyte temperature without introducing possible contamination to the electrolyte.

The cathode is Pd (99.9%), with thicknesses ranging from from .025 - 0.5 mm. One problem with using Pd foils as a part of the cell wall (or figuratively as a cell window) is the possibility of hydrogen gas leaking out from the back surface of the cathode into the air. To prevent this leakage, dense Au films were sputter deposited on the back side of the Pd (the side away from the electrolyte). The Au was made to adhere firmly to the Pd by evaporating an intermediate layer of Cr, 200 Å, which acted as a glue. These Au films ranged from 1.7 - 6.5 μm in thickness (the effectiveness of these films is discussed later). These films were thin enough to let charged particles pass with modest energy loss (protons at 2 MeV lose 90 keV/μm).

The electrolytic cell holds about 150 cc of electrolyte. The top is closed except for seven 1mm holes to release gas pressure and to allow adjustment of the anode position, see Fig. 1. The electrolyte consisted of various amounts of H_2O (100 %) and D_2O (99.5%), with 0.1 molar LiOD as the electrolyte. The lithium hydroxide was made from Li metal (90% ^6Li and 10% ^7Li) reacted with D_2O. The pH of the electrolyte was measured as 12.4.

Various mixtures of H_2O and D_2O were tested as electrolytic solutions because conventional nuclear theory suggests that cold fusion reactions of d + p can have much greater cross-sections than d + d reactions. Since both Ref (1) and (2) did not use pure D_2O, it was possible that they were seeing products of d + p reactions instead of the d + d reactions they presumed. It was felt that a mixture of H_2O (10%) with D_2O (90%) would give comparable amounts of ^1H and ^2H within the Pd cathode metal on the basis of separation factors (5). We used mostly ^6Li for lithium since the cross-section for fusion of d with ^6Li is much higher than for ^7Li. However, the Li metal still contained 10% ^7Li.

The surface barrier detectors were held at a reverse bias of 48 - 75 V to obtain about 100 μm of depletion. This is adequate for the detection of charged particles up to about 8 MeV. Particles with energies above this are still detected but some energy information is lost. The detectors were energy calibrated and tested using a standard ^{241}Am radioactive source.

The anode - cathode current was held at 150 mA/cm^2 for all of the reported studies. Ref. (1) indicates that observed nuclear products scale with current density, so we selected a current density near the high end of their range of values.

The above describes our experiment to detect CNF particles. All particle detectors have backgrounds, either due to trace radioactive materials in their fabrication, or due to cosmic rays interacting with the silicon. Ref. (3) reviews the background sources in silicon detectors and shows typical cosmic ray background rates. The rate we observed was statistically within the rate of this study. We note that we define "Background" as the spectrum observed with our experimental arrangement but with the cell current off. This assumes that there were no CNF particles generated while the cell was not in operation. Only one test was made of Background both before and after operation of the cell, and no change was observed.

An "incubation" or "charging" period for the cathode samples was speculated to be due to the long period which might be necessary for Li to penetrate into the Pd. We tested this possibility by artificially injecting Li into two of the Pd cathodes. In one case Li was introduced by capsule diffusion at 600 C. A second cathode was ion implanted with Li; half the cathode was implanted with ^{6}Li (200 keV) to a dose of 10^{15} Li/cm^2, and the other half was implanted to 10^{16} Li/cm^2. Since we were looking for ANY kind of fusion, it was felt that having a mixed concentration of Li in the cathode would not compromise the experimental results.

Next to the cells were standard biological neutron monitors, and gamma detectors. These were used for personnel safety, and had thresholds of 0.1 mRem/hour.

As an independent evaluation of the "charging" of the Pd cathodes, the crystal structure of the Pd foils was determined by x-ray diffraction, using Zr-filtered Mo K_α radiation and a scintillation detector, with a Θ - 2 Θ diffractometer and symmetrical reflection geometry. Measurements were made for scattering angles, 2 Θ, from 90° to 155°.

EXPERIMENTAL RESULTS

All the experimental results are tabulated in Table 1. In no case was heat detected other than that caused by accountable electrical Joule heating of the electrolyte. Temperatures always remained below 45 C. At no time did the neutron or gamma detectors near the cells register radiation above 0.1 mRem/hour.

One test reported in Table 1 deserves special mention. This study used a 0.5 mm thick foil which was about half the thickness of one of the rods of Ref. (1) and also of the Pd sheet examined in Ref. (1). This cathode had a backing of 6.3 μm of Au. The sample was visually examined after the radiation measurement, and it showed a pronounced bulge, an indication that it was under large compressive stress after electrolysis.

X-ray diffraction measurements were made on the Pd surface of this 0.5 mm thick cathode after the experiment was completed (removing the foil from the cell). See Figure 3. The diffraction signal was characteristic of pure β - Pd, with a lattice parameter of 4.03 Å, only slightly greater than the published value for Pd monohydride, 4.02 Å. There was no evidence for any α - Pd in the diffraction spectrum. This result indicates that 6.3um of dense Au provided an adequate seal to the back surface of the Pd cathode.

The thinner Pd cathodes in our tests had a backing of only 1.7 μm of Au, and one of these was measured with x-ray diffraction. It showed a lattice parameter of 3.92 Å, somewhat larger than the published value for pure Pd (α - Pd), 3.89 Å. There was no evidence of β - Pd in this sample. We speculate that either the 1.7 μm of Au was not adequate to seal the back surface of the foil, thus preventing the β phase from forming, or any β phase present reverted rapidly back to the α - phase. It is not clear whether the formation of β - Pd is essential for CNF, so we report results from both types of samples.

One sample was pre-annealed to 900 C in a vacuum of 10^{-6} Torr for one hour. This anneal was intended to drive out any pre-absorbed hydrogen. The sample showed no unusual results.

CONCLUSIONS

Our experiment measured CNF particles with better sensitivity than that of (1) and (2). Our cell configuration was not like either of those of (1) or (2) since our cathode formed a thin window to allow the exiting of energetic charged particles. Our results show that no excess heat and no nuclear fusion products were detected for any of the samples tested. The partial substitution of H_2O for D_2O did not produce nuclear fusion products. Pre-annealing of the Pd cathode had no effect. The artificial introduction of ^6Li into the Pd cathode by either diffusion or ion implantation had no effect.

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TABLE 1

EXPERIMENTAL RESULTS

Sample Description	Electrolyte (D_2O/H_2O)	Duration of Run hrs.	Fusion Rate (Fusions/cm^3 sec)
F. & P. (Ref. 1) Gamma Rays	Elec. # 1		4000
F. & P. (Ref. 1) Observed Heat	Elec. # 1		10^{12}
Jones et al. (Ref. 2) Neutrons	Elec. # 2		0.7
			Particles detected (/cm^3 sec)
25 μm Pd + 1.7 μm Au Prebaked: 900C/1 hour/10^{-6} Torr Pure α-Pd after testing	Elec. # 1 (100/0)	205	< 0.0038
0.5 mm Pd + 6.3 μm Au Pure β-Pd after testing	Elec. # 3 (90/10)	24	< 0.00092
25 μm Pd + 1.7 μm Au	Elec. # 3 (90/10)	24	< 0.055
25 μm Pd + 1.7 μm Au Pure H_2O Electrolyte	Elec. # 4 (0/100)	24	< 0.050
25 μm Pd + 1.7 μm Au Implanted with 10^{16} ^{6}Li/cm^2	Elec. # 3 (90/10)	24	< 0.036
25 μm Pd + 6.3 μm Au Diffused with ^{6}Li	Elec. # 3 (90/10)	24	< 0.0011

Electrolytes:

Elec. # 1 = 100 % D_2O with 0.1 molar LiOD.
Elec. # 2 = Electrolyte based on components of Earth's Mantle + Volcano Lava (see Ref. 2).
Elec. # 3 = 90 % D_2O + 10 % H_2O with 0.1 molar LiOD.
Elec. # 4 = 100 % H_2O with 0.1 molar LiOD.

Backings:

Pd foils were backed with Au films on the side away from electrolyte to prevent outdiffusion of hydrogen from the cathode (see text). Table 1 indicates the thickness of the sputter deposited dense Au films.

Calculation of detection limit:

In all cases the signal counts (detected particles from 1 - 3 MeV) were within 3 σ of the background counts (taken with the cell current off), assuming Poisson statistics. The quoted particle detection rate is an upper limit based on two σ of the Background counts (90% confidence limit) divided by the cathode volume.

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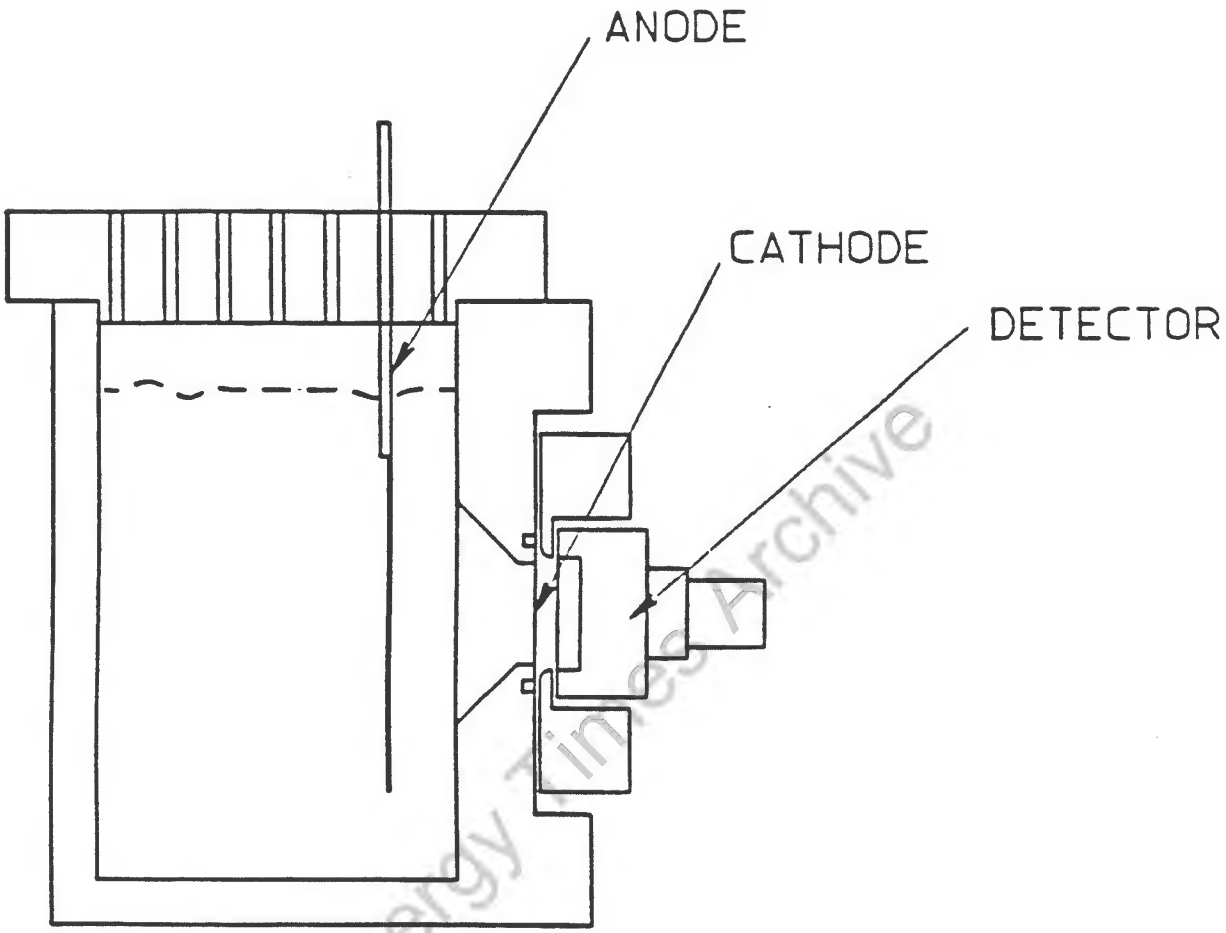

Figure 1

Cross-section of the teflon electrolytic cell. The anode was always Pt, about 2 cm wide and 4 cm long. A 7cm^2 conical hole in the side of the cell led to the 2cm^2 cathode. The anode - cathode spacing was adjustable, but all tests reported here were at a spacing of 2 cm. The cathode was Pd foil of various thicknesses. About 2 mm behind the cathode was a surface barrier detector to measure energetic charged particles. The detector had a background for charged particles with energies of 1 - 3 MeV of about 5 counts / day (mostly due to cosmic rays).

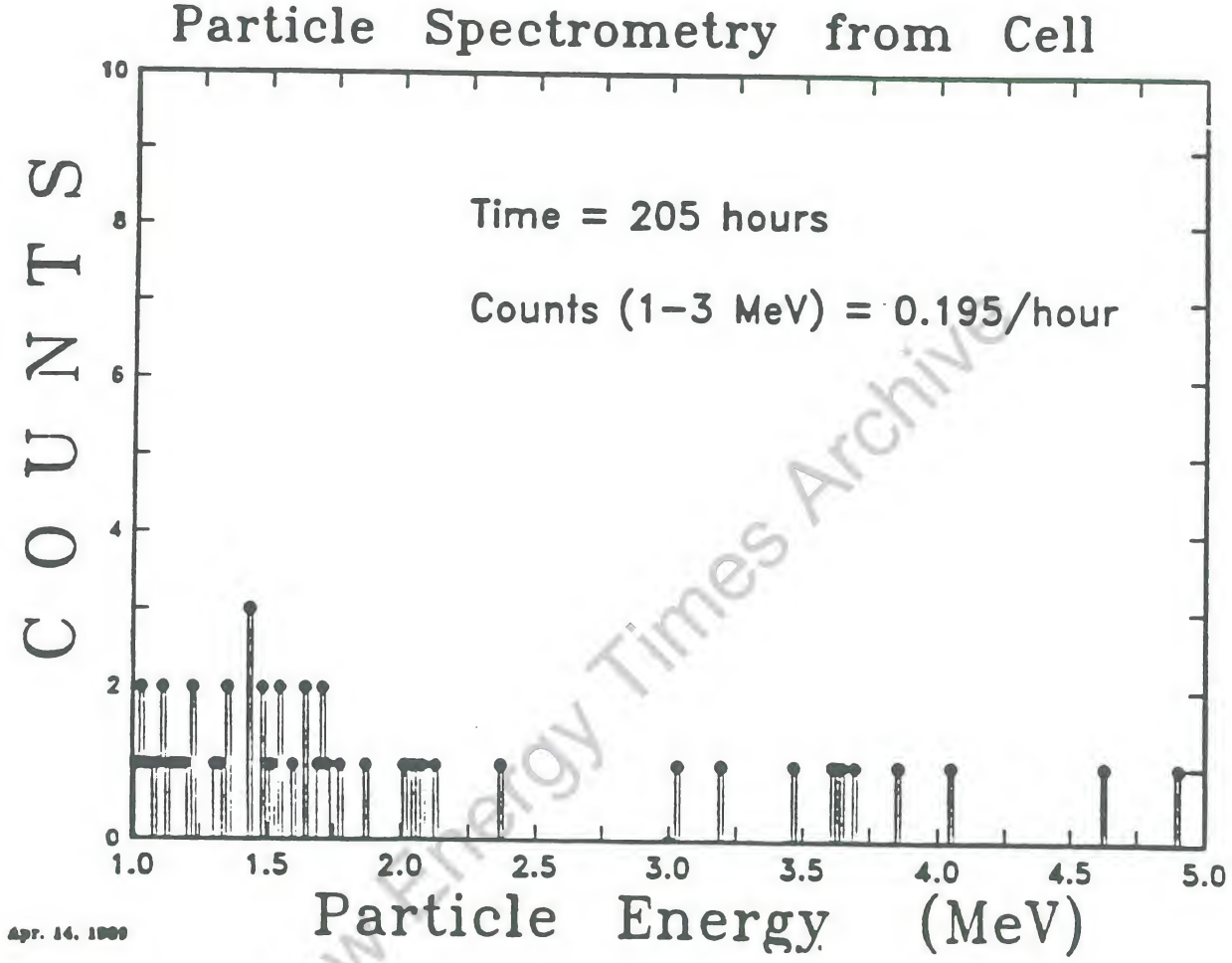

Figure 2

Charged particle spectrum for a thin Pd cathode (25 μm thick, backed with 1.76 μm Au) during 205 hours of charging. Within statistics, all the counts are due to cosmic ray neutrons interacting with the silicon detector, see Ref. (4). The background of the detector was determined by making no experimental changes except for turning off the cell current. This background, taken over 82 hours, was virtually identical to the spectrum with the cell operating. The upper limit for the fusion rate for this experiment was .0038 fusions/cm^{2}sec.

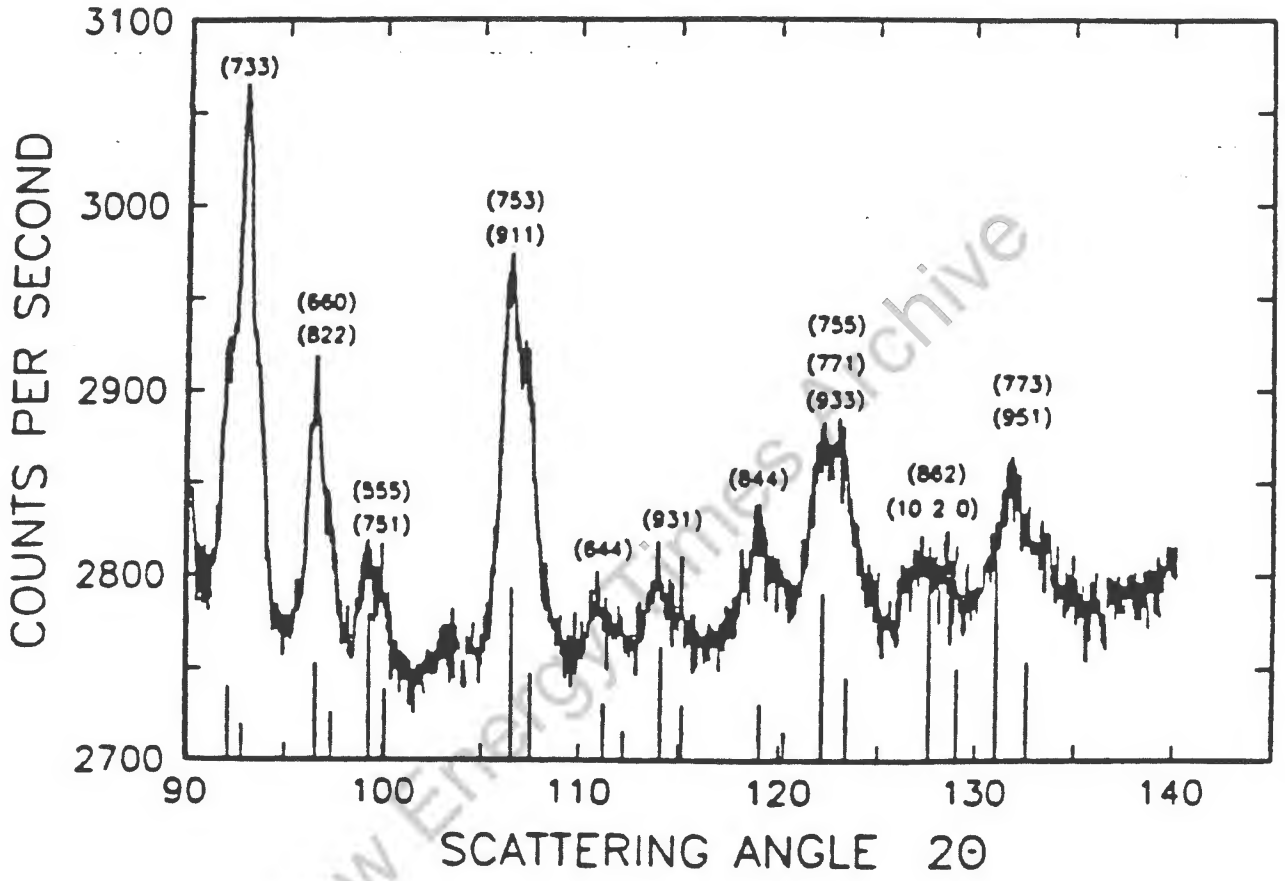

Figure 3

X-ray diffraction spectrum from the 0.5 mm thick Pd cathode, backed by 6.3 μm Au. The peaks correspond to a lattice parameter of 4.03 Å, only slightly larger than the published value for Pd monohydride, 4.02 Å. There is no evidence of the original α - Pd metal in the spectrum. This leads us to conclude that complete hydrogenation of the cathode was obtained within the limits of the applied voltage and current density.